

Psychological Bulletin

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THE PSYCHOLOGICAL BULLETIN

SLEEP

BY H. M. JOHNSON AND T. H. SWAN

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The studies which are covered in this review may be grouped as follows: (1) those which employ the method of sensory stimulation; (2) those which employ registration of changes in bodily position; (3) those which employ measurement of the rate of oxygen-consumption; (4) those which depend primarily on histological examination of tissues; (5) some theoretical contributions; and (6) a few miscellanies.

1. *Studies based on the method of sensory stimulation*

This method is probably the first one that was used in a controlled experiment. Fundamentally it consists in measuring the magnitude of the stimulus which is found necessary for awakening the sleeper, the criterion of *awakening* being arbitrary, but in theory, constant. The results based on this method form the basis of most of the discussion of sleep to be found in the medical, physiological, and psychological textbooks.

As a preliminary to the publication of three years' experimental work, based on an entirely different method, the present reviewers have made complete written translations of the original reports of Kohlschütter (20), Mönninghof and Piesbergen (30), Michelson (29), and Czerny (5); and have availed themselves of the translation of the study of de Sanctis and Neyroz (34) made by Warren. They have checked the original exhibits of the data, compared them with the texts of the reports, and in some cases have reduced the data to other forms of description for the better com-

parison of the results. This procedure disclosed that the current works on sleep, including special treatises as well as textbooks, abound in misstatements of fact concerning the studies just named, as well as in uncritical and sometimes preposterous interpretation. The explanation may lie in the fact (which is rendered clear from internal evidence) that most of the comparatively recent writers have not read the series of reports which they cite, but have depended on the accounts of other authors, who likewise have not read them. We shall not enter into detail here concerning the history of some of these errors, although some of them are easily traced, and the recital would make an interesting story. However, the fact that some of the original reports are now hard to get, together with the fact that they deal with important questions, makes it seem profitable to publish in 1930 a review of work begun in 1859, although the reports can hardly be classed as current literature.

Certain of these studies (20, 29, 30) were the dissertational experiments of candidates for the degree of doctor of medicine; others were made by young doctors during their service as internes and assistants. Possibly the later engrossment of the authors in the details of medical practice may account for their failure to follow up some of the exceedingly interesting questions which they propose, during the discussion of their experimental results, as problems for future attack.

The background of this group of studies was set by Kohlschütter (21), who was inspired by his teacher Fechner (11). It is interesting, whether pertinent or not, to note that the results which Kohlschütter presents in the form of an ideal curve of the "depth of sleep" (so frequently reproduced in textbooks and dissertations) fit very accurately the *a priori* description given by Fechner (11, II, 433 ff) of the diurnal course of the fluctuations of "general consciousness." We shall presently see that this concordance was accomplished by Kohlschütter's disregard, on very questionable grounds, of numerous measurements which deviated widely from the expectation which Fechner had raised.

All these studies rest upon three considerations, some of which are not expressly stated, though definitely implied. We shall attempt to state them; not in the exact form which the original authors would have used, but in one which, at some points, is more simple than Fechnerian terminology permits.

- (1) The notions of *sleep* and of *waking* are strictly *relative*;

first, to the particular *stimuli* which are being considered; and next, to some particular *kind of response*, which is *arbitrarily* taken as an indicator of the efficacy of those stimuli. Corresponding to each presentation, the experimenter enters a categorical judgment: *i.e.*, that the critical response occurred, or that it did not.

Other conditions being constant, the less work a stimulus has to perform on the patient's sense-organs to effect the critical response, the more *awake* the patient may be said to be, *with respect to that particular stimulus*. If the minimum effective input, under standard conditions is b , and under compared conditions is b' , the relative degree of waking (*with respect to the particular stimuli employed*) may be defined as some function of b and b' , and its negative treated as the "depth of sleep," with respect to the same stimuli. It is important that the qualifying phrases be preserved, even at the risk of tedium, for their disregard has occasioned grave confusion in thought and discourse for some 68 years.

We may further observe that such a treatment yields only a fact of definition: it provides us with an additional way of talking about the facts of observation, a way which resembles the every-day manner of speaking, but that is all.

Recalling that "sensitivity," "*Empfindlichkeit*," "irritability," "responsivity," *etc.*, are names given to some function or functions of the ratio between the *magnitude*¹ and the *effectiveness*² of the stimulus concerned, we may relate the two facts of definition and identify the degree of waking with some function of the sensitivity of the subject or the "effectivity" of a standard stimulus; and call the negative of this formal entity the "depth of sleep," with respect to the stimulus in question.

These definitions, though unnecessary, are respectable, and for some purposes may be convenient. But, they denote nothing, not even the existence, of *introspectively observable sensations*, until such a reference is attached by an additional *assumption*. That assumption is the subject of our next consideration.

(2) Fechner defined³ the intensity γ of the consciousness aroused by a particular stimulus, as follows:

$$\gamma = k \log (\beta/b)$$

¹ Measured in *physical units*.

² Determined by *classifying responses* according to some conventional standard, *counting* the responses in each class, and expressing the numbers relatively.

³ Fechner often referred to this relationship as being empirically derived.

in which k is a constant of proportionality, β the magnitude of the stimulus, and b that value of β which has standard effectiveness in the sense given in our preceding footnote. If $\beta < b$, then $(\beta/b) < 1$; its logarithm is negative, so that the intensity γ of the corresponding item of consciousness is also less than "zero." This "zero," however, is not an absolute *null*, indicating a complete absence of *sensation*; it is merely an arbitrary reference-value. As Kohlschütter observes (20, 210ff.), the use of plus and minus signs means simply that the magnitude of γ exceeds or falls short of an arbitrarily chosen, finite value, by the amount indicated by the numerical part of the expression. A negative value of γ indicates *sleep*, in Fechnerianism, with respect to the stimulus in question. In plain words, it means that the stimulus is too small or too feeble, at the time, to arouse the conventional response as frequently as an arbitrary standard requires. Such a condition could occur from diminishing the value of β or from diminishing the patient's sensitivity and thus increasing the value of b . This interpretation summarizes all the empirical facts which are covered by Fechner's description of sleep as a condition of "*negative consciousness*"; although that description, being an obscure utterance of a great man, has often been treated as profound. All the empirical facts presented by Kohlschütter could have been stated fully and yet simply, without reference to the assumption implied in the *Massformel*. The fact is important, for the application of the latter formula to "*general*" consciousness as well as to the kind of *sensation* which it defines, rests upon still another assumption, which more recent experiment has shown to be invalid: namely, that the energy released as a result of the action of the stimulus upon the organism is proportional to the magnitude of the stimulus! Whatever validity the results of Kohlschütter *et al.* may have, can be more readily determined if the results are considered independently of the invalid assumption that determined the choice of descriptive terms.

(3) If the "depth of sleep," defined according to consideration (1), and taken with respect to a particular complex of stimuli, be treated as if it represented the depth of "*general sleep*," the treatment implies an assumption: namely, that in sleep, the "*effectivities*"

It is, in fact, based on an *interpretative* statement of Weber's law, which proves from analysis to be a defining equation and not an empirical relationship. This analysis is presented in detail in an article entitled "Did Fechner Measure 'Introspectional' Sensations," by one of the present writers (J), published in the *Psychological Review* in 1929.

of all stimuli diminish in the same proportion, or that one's sensitivity to all stimuli has diminished in equal degree. This assumption is explicitly denied by Fechner and also by Kohlschütter. It is restored by implication, however, by Kohlschütter, Mönninghof and Piesbergen, Michelson, and Czerny, for while none of them pretended to measure anything but the varying depth of sleep with respect to particular noises or electrical currents, all of them discuss the results as though they exhibit the variations of "general" sleep.

To express the "depth," at any moment, of what these definitions would require us to call "*general sleep*," it would be necessary to measure, separately and simultaneously, the effectivity of each member of the universe of stimuli that are playing on the organism at the moment, and to weight and combine the numerical expressions of the measurements in a form which could be stated. The procedure would have to be repeated for each temporal instant included in the comparison. Since fulfillment of these conditions is physically impossible, we may be assured that for practical purposes, there is no such thing as "*general sleep*," in a sense which satisfies Fechner's definitions.

The more recent writers refer to the experimental studies which we have listed above as if they were made on one and the same variable. This treatment appears to arise from the fact, that each of the authors, having obtained a series of numbers which expressed the results of measurements of *stimuli*, named the numbers expressions of the "*depth of sleep*."⁴

As a matter of fact, each of these studies was confined to a single sense-field, with a single type of stimulus appropriate to that field. Moreover, in the three studies (20, 30, 29) which employed the special sense of hearing, the values presented by the different authors do not even represent the products of the same dimensions. This point is important to proper interpretation of the results; we shall therefore enter into a few details for the sake of developing it.

⁴ The fallacy of treating two variables as identical because they happen to be called by the same name is known in Logic as *Equivocation*. It occurs very frequently in the literature of all branches of endeavor which do not rigorously follow the rules of scientific method. It curses the descriptions and the teachings of medicine, physiology and psychology; it forms the basis of many of the so-called 'problems' of philosophy; it generates most of the absurdities of the mental testing movement, as well as the commercial success of the latter. Its use is a grave immorality, which requires careful watching for prevention and detection.

Kohlschütter (20) employed a sound-pendulum constructed by Fechner. It is not, however, either of the two which Fechner describes in his *Elemente* (11). Kohlschütter's pendulum having been raised to a height indicated on a graduated arc and released, its bob, or hammer, struck against a block of slate at the bottom. The weight and material of the hammer and the dimensions of the block of slate are not given: hence, one could not hope to reproduce, even crudely, the characteristics of the sound-wave which the apparatus generated. The location of the apparatus was not equally distant from the two ears of the observer. Kohlschütter found that the auditory acuity was not the same for the two ears. He therefore segregated the results according as the right or the left ear was nearer to the instrument, and took account, in a way to be mentioned presently, of the distance of the instrument from the nearer-lying ear. He disregarded, however, the angle at which the external opening in the ear was inclined to the direction of the sound-wave; and also disregarded whether the one or the other ear might be occluded, as by the pillow.

The angle of elevation ψ being read directly, the height of fall, h , may be derived from the identity given in elementary Geometry:⁵

$$h \equiv 1 - \cos \phi \equiv 2 \sin^2 \frac{1}{2} \phi$$

The total energy of impact is given by Mh , in which M represents the mass of the hammer, and h the height of fall. Kohlschütter assumed that a constant fraction of the energy of impact is converted into sound. Vierordt, a physiologist, disputed this assumption, and in a private communication to his pupils Mönninghof and Piesbergen, presented experimental evidence, the character of which they did not give, to support his criticism. Our colleagues in physics inform us that the settlement of the question would be extremely difficult by experimental methods now available. It would have been still more difficult of settlement by the methods which were available in Vierordt's day. Kohlschütter assumed further that the energy delivered by the sound-wave to the ear varies inversely as the square of the distance between the source and the ear, even in the closed room in which he worked. Obviously, the energy in the primary wave would be augmented by reflection from the walls, etc. As he could not ascertain how great this augmentation was, he assumed it

⁵ This equation is misprinted in Fechner's discussion, and is so badly set in Kohlschütter's article as to invite misreading by one who did not pause to reason the matter out (11, 180; 20, 214 f.).

to be zero, thus exemplifying another fashion in procedure which experimentation sometimes justifies and sometimes overthrows. (If the augmentation factor were constant, it would not affect the relative values of the results. It would be constant if the positions of the ear and of the generator were constant with respect to the walls, floor and ceiling of the room, as well as with respect to each other, and if the energy-frequency distribution in the sound-waves were independent of the height of fall, or else the absorption of the walls, etc., were non-selective.)

On the basis of facts and assumptions combined, Kohlschütter reasons that his stimulus-intensities, measured by the work performed on the ear-drums of the patient, were proportional to

$$I = (288 \sin^2 \frac{1}{2} \phi) / e^2$$

in which e represents the distance of the block of slate from the nearer ear, measured in the Leipzig foot which contains 31.3 cm. The factor 288 should be proportional to the weight of the pendulum-hammer.

Mönninghof and Piesbergen (30) employed as a sound-generator a leaden ball, flattened in several places to prevent rolling. It weighed 16.2 grams, and fell freely from a variable height upon an iron plate $20.4 \times 16.7 \times 0.55$ cm. They assumed that the energy converted into sound was not proportional to the product Mh of the mass and the height of fall, but proportional to $Mh^{0.59}$. For the value of the exponent they relied on the communication of Vierordt, mentioned above. They also assumed that the energy delivered to the receiving surface varied, not as the inverse of the square of the distance from the source, but inversely as the first power of the distance. As the mean distance they employed was 150 cm., they considered such deviations to be negligible as might be introduced by the patient's changes of bodily position, and therefore took no account of them. They also disregarded whether one ear or the other was nearer the source of sound, and also whether one or both ears may have been covered, and what might be the angle of inclination of the effective ear with respect to the direction of the sound-wave. Four threshold-determinations which they made on each other yielded a mean value of 34.13 for the product $Mh^{0.59}$, in which M was measured in milligrams and h in millimeters. Desiring to express their results relative to this mean threshold, they called their variable products $Mh^{0.59}/34.13$ measures of sound-intensities.

Michelson (29) used 14 brass balls, whose weights ranged between 5 and 100 grams. Despite the presence of variables which might effect a change in the order of magnitude of a threshold determination, he expresses the observed weights in milligrams, or to four places of decimals in one extreme case and to six places in the other. He selected and released these balls by means of an ingenious electro-mechanical device which operated without noise and which was controlled from a distant room. The balls fell upon a sounding-board of oak, $25 \times 25 \times 3.5$ cm., inclined to the line of fall, so that after impact, the ball was reflected into a padded box. The mean distance from the patient was 100 cm., which was treated as constant. Disregarding all other factors, Michelson assumed the sound-intensities to be proportional to the product Mh , expressed in grams and centimeters respectively.

From the foregoing description it can be seen that there is now no reliable means of reducing these three sets of results to a comparable basis.

Czerny (5) worked on children, using electrical stimuli produced by a DuBois-Reymond inductorium. The secondary coil was fixed with reference to the primary. The current through the latter was supplied by a battery of LeClanché cells, varied by means of an adjustable resistance, and measured on a milliammeter. The child was made part of the secondary circuit by means of flexible connectors which terminated in metallic zinc plates, secured to the upper arms of the child by inelastic bands. An extra switch permitted the secondary circuit to be shorted around the child while the preparations were being made. The latter being closed, the primary circuit was closed, the rheostat adjusted to give a predetermined reading, and the short-circuiting switch opened. The primary circuit was then broken by a manual switch. The procedure was repeated with progressive increases in the primary current-strength until the child made a response which the experimenter regarded as critical.

It has been mentioned that the primary current was measured during the period of steady flow. Czerny assumed that the strength of the secondary current induced by the break was proportional to the strength of the steady primary current. He therefore presented the numerical expressions of the latter as relative measurements of his *stimulus-intensities*. As far as we have yet seen, this interpretation has not been questioned by the numerous writers who refer to

his experimental report. If a little regard be paid to the physical conditions, the interpretation will prove to be invalid.

Under the conditions described, the intensity of the actual *stimulus* applied would be proportional to the *density*⁶ of the current which passes through the receptors in the very brief time during which the inductive discharge takes place.

This would be affected to a degree which is worthy of a first order of consideration, by three conditions which were actually variable, but which Czerny treated as constant.

(1) The first condition is the *effective area of contact*. The form of the upper arm does not remain constant; that of the electrodes does. The area of contact depends on the area of skin which conforms to the metal. The conformation may be altered in two ways: (a) by changes in the form of the arm which accompany changes in posture; and (b) by changes in the volume of the arm, for if the arm swells, a larger area of skin is brought into intimate contact with the metal, whereas this area is diminished when the arm shrinks. (It will be recalled that the bands which fastened the plates to the arm were inelastic.) It is easy to see that the form of muscular tissue does not remain the same for all postures, and Howell (14) has shown that the volume undergoes marked fluctuations during the time devoted to sleep. A constant *current* if sent through a large area of skin, containing many receptors, would yield a smaller *current-density* than if it were sent through a smaller area, containing fewer receptors. If the *current* is comparatively weak, the result, in the first case, might be subliminal stimulation of many receptors, resulting in failure of the critical response; and in the second case, the effective stimulation of fewer receptors, resulting in production of the critical response.

(2) The second condition is the resistance between the skin and the electrodes. This factor would be affected by two agents which are known to vary, not to speak of others, such as film-surfaces, of which we know but little. (a) The intimacy of contact between skin and electrode partially determines the resistance, so does (b) the moisture of the skin and the concentration of salts in the film of sweat between skin and metal. Of the variations in the former during the course of the experiment, we know nothing except that they were not prevented and should not be ignored. Of the latter,

⁶ Measured in amperes per square centimeter.

as we shall presently see, Czerny has told us something, of which he took no adequate account.

(3) The third condition is the leakage by conduction over the *surface* of the skin. This leakage would be small if the skin-surface which separated the two electrodes were dry: but it would certainly increase if the skin were rendered moist by perspiration. Czerny actually measured the rate of perspiration of the skin of the forehead in two experiments, and presents the results. The latter may not reliably indicate the rate of perspiration in the skin of the arms and trunk, or enable us to estimate the humidity of the latter, but they do show us that in the skin of the forehead, the rate is by no means constant. In one experiment [17], the range of variation was of the order of 76 to 1; in the other experiment [18], of the order of 62 to 1. *The heaviest currents through the primary coil were required for awakening at those times of the night when the rate of perspiration was the highest, and conversely.* Czerny's interpretation of this finding is that the stimulus-intensities remain constant, but that the patient became less sensitive when the skin was moist: our suggestion is that under such conditions the intensities themselves were reduced by leakage more than they were increased by diminished resistance through the skin. We do not argue that the leakage was sufficient to account for all the differences which Czerny got; in fact, we suspect the contrary from considerations independent of his experiment; but the point can be settled only by direct measurement. But we do contend that to assume that the stimulus-intensities were simply proportional to the primary current-strengths is to neglect some variable *factors* which cannot be safely disregarded; their consideration might greatly change the form of the curves employed to express the results.

In this connection we may remark also that the key in the primary circuit was opened by hand; and that the characteristics of the current induced by the break are by no means independent of the speed with which the two contacts are separated.

De Sanctis and Neyroz (34) employed pressure-stimuli, obtained from the blunt point of an aesthesiometer, applied to the patient's forehead. The total time allowed for each stimulation was 20 seconds; the experimenter noted the position of the pointer at the time of occurrence of the first non-specific response, such as a facial contortion, and of a response considered as appropriate. Thus, in addition to the uncertainty which attends the reading of a moving

pointer while one is also watching something else, the readings were subject to the variability of the patient's and the experimenter's reaction-time. These authors plotted these readings under the label "depth of sleep."

In respect to *experimental procedure*, this group of studies presents an equally interesting divergency.

Kohlschütter awoke his patients as often as seventeen times during a single night. Beginning with an ineffective angle of elevation of the pendulum-hammer, he caused the latter to strike six times upon the block, at intervals of one second. If the critical response did not occur, he increased the angle by 5° and repeated the procedure, thus continuing until he found an effective elevation. He encountered the obvious criticism that such frequent application of the stimulus must affect the natural course of sleep. He admitted that this defect is inherent in the procedure; but he discounted it in some degree by arguing that most of the stimuli were subliminal, and that it is impossible that a subliminal stimulus, however oft repeated, can summate to effectiveness.⁷ His argument is now well known to be inconsistent with fact. Despite his insistence, however, he actually took some account of effects of previous stimulation in his manner of *weighting* the maximum ineffective and the minimum effective stimulus-values in computing the threshold. Among the effects which influenced the weighting were changes in the character of respiration. Reed and Kleitman (33) report that such changes are unrelated to changes in specific sensitivity.⁸

Kohlschütter's work has been criticized on the further ground that the presence of the experimenter in the sleeping room, and the

⁷ "Eine Summierung der in der Zeit getrennten, für sich unzureichenden Eindrücke zu einem stärkeren findet hierbei nicht Statt. Denn dass ein noch so häufiges und noch so rasches Wiederholen einer an sich zu geringen Intensität nicht Erwachen bewirken kann, davon habe ich mich durch specielle Versuche überzeugt. Man kann eine solche fünfzig- bis hundertmal in rascherem oder langsamerem Rhythmus einwirken lassen, ohne dass Erwachen erfolgt. Selbst ganz nahe an der Grenze vermag die öftere Wiederholung nicht die absolute Insuffizienz auszugleichen" (20, 220).

⁸ Michelson misrepresents Kohlschütter's procedure in this respect, declaring that the former accepted very slight changes in respiration as signs that the patient had awakened. Kohlschütter (20, 218) tells us: "Als eigentliches Kriterium des Erwachens habe ich aber nur die Laute welche vorhergangener Verabredung gemäss der Geweckte ausstieß, betrachtet." Michelson's error has been propagated by later authors, who appear to have relied on his account instead of consulting the original.

illumination in the latter, may have introduced still more disturbance. The same objectionable features attended the work of Mönninghof and Piesbergen (30), de Sanctis and Neyroz (34), and apparently also of Czerny (5).

Mönninghof and Piesbergen (30) presented three sets of stimulus-values for each single determination: namely, (a) the minimum found to be effective; (b) the sum of those found to be ineffective; and (c) the ratio a/b . It is thus evident that they did not measure thresholds, although, since they proceeded by equal increments of height, they left the calculation of threshold-values possible. They presented each stimulus but twice, and made not more than two experiments on any one night. By so doing they obviated some objectionable features of Kohlschütter's work. Their criterion of awakening was a predetermined signal.

Michelson (29) was able to keep his sleeping chamber dark, since the selection and the determination of his stimuli were controlled from another room. His criterion of awakening was the pressing of a key, above the patient's head, so that the patient had to raise himself up in order to reach it. This key operated an electromagnetic signal in the experimenter's room. The patient was instructed to give a special signal in case his response was delayed after awakening. Michelson presents the mean of the minimum effective and the maximum ineffective values, where both were obtained.

Czerny (5), as was mentioned above, did not measure thresholds, but presents only the minimum values which he found to be effective. His criterion was variable. He accepted outcries, weeping, assertions like "*it hurts*," or "*it sticks*," facial contortions, visual fixation of the experimenter, and changes of bodily position. The conditions of the experiment required the room to be lighted sufficiently to enable him to see the child. He awoke his patients as often as ten times in one night, at intervals of one hour, if the patients slept so long.

De Sanctis and Neyroz (34) did not measure thresholds, but presented two sets of readings on the scale of the *æsthesiometer*, corresponding to the two types of response mentioned above. They read the scale by means of a small spot of light projected upon the scale, and upon the forehead but not upon the eyes of the sleeper. They aroused the patient not more than twice in one night. When he awoke, the experimenter questioned him concerning his dreams. It sometimes happened that the maximum pressure which could be

read on the scale did not produce awakening. In such an event, the instrument was withdrawn and the stimulation repeated. The sum of these readings was then taken as the effective intensity: on what ground we were unable to make out from the report, nor did we succeed in imagining any which were valid.

The foregoing discussion will indicate that the series of numbers which the authors present, do not, in any case, express the threshold stimulus-intensities, or even a measure of the intensities whether liminal or not. It is rather difficult, in fact, to assign any meaning to them which one may regard as certain. Nevertheless, when considered as functions of time, some of them present an interesting correspondence with data which are interpretable and reliable; so that one cannot regard the correspondence as accidental.

Kohlschütter (20) presents a curve obtained on one subject covering a series of eight experimental nights, and based on a total of 74 determinations. It shows that the height from which the pendulum-hammer had to fall in order to elicit the required signal from the subject, varied according to the time which elapsed after falling asleep. It increased very rapidly until the end of the first hour, after which, according to the curve, it declines after the manner of a logarithmic curve of decay.⁹ The ordinates of this curve are labelled *Festigkeit des Schlafes*, an expression which he uses interchangeably with *Schlaf tiefe*, and which is therefore frequently rendered "depth of sleep." Three interesting points have to be considered in evaluating this curve.

First, both he and later writers have treated the curve as if it represented the course of "general" sleep; whereas all that Kohlschütter attempted to measure was the course of the depth of sleep with respect to a particular sound stimulus. In fact, he explicitly denies, in another place, that all functions are proportionately depressed in the same epoch of "sleep."

Secondly, if one regards sleep as the product of two dimensions, namely intensity I and duration T , and accepts this curve as showing, ideally, the distribution of the intensity of sleep, some important consequences follow. The quantity taken between any two temporal instants t and t' is therefore expressed by the product-sum $\sum_{t'}^t \Delta I \cdot \Delta T$. An approximate integration, effected by a graphical

⁹ The general formula for the descending limb of this curve is $y = pe^{a/a-a}$. It is incorrectly given by Kohlschütter (20, 248); probably through a printer's error, the exponent of e is shown as a coefficient.

method, of the values given, shows that about 55 per cent of the total quantity of "sleep" is taken within the first hour, about 82 per cent during the first $1\frac{1}{2}$ hours, and about 92 per cent within the first two hours. This circumstance alone should have sufficed to raise the question whether the kind of "sleep" which is defined in these terms and represented by this curve is the same kind which Kohlschütter really set out to measure. The point seems almost to have escaped mention: one author, indeed, remarked that the recuperative effects of sleep do not seem to be proportional to the quantity taken, but usually this presentation is taken at face value. Our survey of popular as well as of scientific literature disclosed frequent instances in which some eminent authority increased his eminence by advising the public to take its sleep in broken doses, not exceeding two hours in extent. Wherever we could find an allusion to a factual warrant for such advice, it appeared to lie in a very casual consideration of Kohlschütter's curve.

Finally, this curve fits nicely the description given by Fechner (11, II, 33) of the fluctuation of "general consciousness" during this part of the 24-hour cycle. This description is not based on any measurements, but is rather fanciful. As a matter of fact, however, the descending limb of Kohlschütter's curve does not express the numbers in the corresponding table at all; for he obtained numerous readings after the first two hours which are higher than any point on this branch of the curve, some of them being about as high as his first maximum. All these readings, which constitute over 40 per cent of the total number, he rejects. In some cases he gives no reason at all, but in over half the cases he assigns the following reason: namely, that the patient had "recently" stirred, so that the measurement was made shortly after a secondary *Einschlafen*, and therefore represented a point near the beginning of a new curve. His criterion of recency was fairly liberal, including half an hour at least, and there is also some reason for suspecting that it may have been elastic. The unpublished experimental results of Johnson and Weigand show that in a typical subject, about one-third of the total time spent in bed is interrupted by stirring every ten minutes or oftener, and about two-thirds of it is interrupted by stirring every 30 minutes or oftener. If Kohlschütter's patient slept much like this one, he could have rejected from two to three times as many measurements as he did reject on the ground assigned. One therefore suspects that if the result conformed to expectation, he retained it

despite the recency of stirring, arguing, perhaps, that the influence of the stir had worn off. If he had considered all his measurements, the descending limb of the curve would have shown several peaks, some of which would have been quite high.

This sort of thing is in fact what Michelson and de Sanctis and Neyroz found: large fluctuations, occurring in an order which is almost rhythmic. They also characterize the curves obtained by Howell (14), which show the temporal changes in the volume of the arm during the time devoted to sleep.¹⁰ They also characterize the curves showing the distribution of motility over the time spent in bed, as obtained by Johnson and Weigand on some 75 adults and by C. R. Garvey on 12 young children, in the course of the Simmons investigation and the studies at the University of Minnesota. Most of these patients were studied over a period of several months, and most of the curves exhibit the result of the examination of several thousand items in the record of the individual subject. While the data of observation in these four experiments were different, they appear in each case as similar functions of time. The tendency or disposition toward immotility, as Szymansky calls it, follows a characteristic rhythm; so, apparently, does the sensitivity of the subject to particular stimuli; so does the relaxation of skeletal musculature. It remains for simultaneous observations to determine whether they all go together, as would appear from *a priori* considerations.

Kohlschütter's elimination of the measurements which showed this tendency therefore seems unwarranted. If the "sleep" which he studied was found to be subject to interruptions and to fluctuations, an "ideal" curve which does not exhibit the fact is not, after all, ideal.

Mönninghof and Piesbergen's (30) curves, showing results obtained on each other, do not show this rhythmic tendency, although all of them have at least two maxima. The first maximum occurs later than Kohlschütter's, as does also the first maximum of Michelson's (29), and of Lambranzi's (25), as presented by Piéron,

¹⁰ These fluctuations he believes to be dependent mainly on the volume of blood in the arm, which he further assumes to represent the complement of the volume in the brain. The *empirical findings* should be distinguished from the interpretation, since the latter is inconsistent with some experimental results obtained since that time. The increased volume of the arm very probably accompanies *muscular relaxation*; the diminished volume, *muscular tension*; so that they may be important elements in the syndrome of sleep, taken by themselves.

the original of which we have not yet been able to obtain. Much discussion has been expended on these divergences. They may be partly due to the diversity of methods employed, and partly to the uncertainties of measurement; but they are not huge as compared with individual differences which were discovered in the Simmons investigation under conditions to which these suggestions are much less applicable.

Czerny's results (5) also show two maxima, one in the earlier part of the night, and one in the later. They are separated by a minimum, in which the sensitivity of the waking state is very closely approximated. The results show no evidence of rhythmic fluctuations of irritability, which is a striking characteristic of the results being obtained on the Minnesota children.

In one respect all these reports agree among themselves: all of them show that it is more difficult to awaken the subject in the period between three-quarters of an hour and two hours after his falling asleep, than at any other time. This finding corresponds to Howell's report of his own observations, and save for some individual differences, it is rather well supported by the results of the Simmons investigators.

Certain other findings of these authors will now be listed, with the qualification that they are rather suggested than proved to hold for "*general*" sleep.

Czerny found (5) that the periods of low irritability of children generally fell in the first and the fourth quarters of the night. This finding suggests that their rest is disposed to occur in two distinct segments. The interpretation suggests itself, that the heightened irritability in the middle of the night is a degraded tendency to awaken for midnight feeding: a habit set up during the earliest weeks of infancy, which persisted for a long time thereafter. Michelson (29) found a similar trend in one individual who was mildly asthenic. Karger (16, 502, ref. 10) interprets such behavior as a sign of a neurotic disposition. Concerning this interpretation, the much more voluminous results of Mr. Garvey on young children, and of Dr. Wholey and Mr. Weigand on schizophrenics, should have an important bearing, when published.

Michelson (29), who found that two healthy, robust subjects showed a minimum irritability (with respect to his experimental stimulus) early in the night, and a heightened irritability in the later part, while the asthenic subject delayed his period of minimum

irritability until the last third of the night, remarks that the disorders of sleep, found in certain diseases, may demand quite different soporific agents according to their type. In senility and in the manic phase of manic-depressive insanity, so he asserts, the patient quickly falls asleep, but awakens very early, and has great difficulty in falling asleep again. In such disorders, a soporific is indicated whose action is delayed for some hours after administration. He suggests the use of sulphonal. In asthenias and in the depressive phase of manic-depressive insanity, the patient, so he says, tends to remain restless and irritable until late in the night, the deepest rest occurring toward morning. Such behavior, he considers, would indicate the use of a quickly acting drug, such as paraldehyde. This problem is of first importance, but we consider the method of study inferior to that of registration of motility, combined with statistical description. We shall have more to say concerning it in our discussion of Guttman's (12) report.

Following a dose of paraldehyde, Michelson found that the irritability to his experimental noise was greatly decreased. The effect set in quickly, and persisted for $2\frac{1}{2}$ hours. The experiment was made in the afternoon. He also found that his patients were much more easily awakened by the experimental noises in the spring and summer than in the fall and winter. This finding corresponds to popular belief, and is corroborated by the studies on college men (17) made at Mellon Institute.

Czerny (5) compared records made on children during nights which followed an afternoon nap with records made during nights which did not. Following the nap, the first minimum of irritability to electrical stimulation is slightly postponed; but during the greater part of the night, the patient is harder to awaken than when the nap was omitted. The finding suggests that within limits, the more time the patient devotes to resting, the more quietly will he tend to rest while he is about it. Such an interpretation will shortly receive independent support.

Kohlschütter (20), Mönninghof and Piesbergen (30), and Michelson (29) report that they found it harder to awaken a patient after he had recently stirred and relapsed into quiet, than at ordinary times. It seems probable that certain stimuli produced in the course of the maintenance of posture itself may serve to reinforce the experimental agent of awakening. Among them are stasis of blood and other bodily fluids, interference with circulation, overheating of the

unventilated portions of the skin, the stretching of muscles, the cramping of joints, and the pressure of bed-clothing upon the body, the body upon the mattress, and one visceral organ upon another. In time these conditions must become irritating, and eventually lead to a change of bodily position, by which they are relieved. Shortly before the patient stirs, external stimuli are reinforced by them; for a short time thereafter, a great deal of internal stimulation is removed, so that the external agents must be abnormally increased to produce a given response.

Mönninghof and Piesbergen (30) report that when the experimental night is preceded by unusual exercise, such as long walks, after which a little alcohol (four glasses, or less, of light beer) is taken, the subject became more sensitive than normal with respect to the experimental noise. When exercise was omitted, and much more alcohol taken, the subject became less sensitive to the noise during the earlier portions of the night. They attribute the latter circumstance partly to the fact that the patient stirred more often than usual, so that more of the measurements were made shortly after he had settled than would normally be the case. One would suspect, however, that the increased frequency of stirring might be due to the presence of extraordinary irritation—perhaps in the alimentary tract, the bladder, etc., which would operate as a reinforcing agent unless there was a concurrent reduction of *specificity* of response. We are not well satisfied, therefore, with the explanation proposed. On the factual side, however, we can add a little in corroboration: we have direct evidence that a sleeper stirs considerably more frequently than normal if he is drunk when he goes to bed.

2. *Studies based on registration of changes in bodily position*

The pioneer in the use of this method appears to have been Szymansky (36, 37, 38), whose investigations were begun prior to 1914. The results, of course, are strictly relative to the method, and are meaningless unless the principles and the limitations of the latter are considered along with them.

The essential requirements of obtaining the original data are extremely simple, although Szymansky, as well as Karger (16, 502, ref. 11), Naegele (16, 502, ref. 10), and others, exhibited great ingenuity in designing and constructing their experimental apparatus. The cage, or bed, is mounted in such wise that it yields to the movements of the occupant. It is then connected mechanically or pneu-

matically to a recording instrument, which registers some, though not all, of the components of the movement on a strip of moving paper. (The Simmons investigators have since found that electrical transmission is also feasible.) If the speed of the paper is uniform, the time at which the subject moves may be obtained directly by scaling; otherwise, an independent time-record is necessary. In general, the displacement of the recording pen is not proportional to the work performed in effecting the change of bodily position. This assertion is based on the fact that all the mountings were subject to periods of their own, so that the displacement of the pen depended in part on the phase in which the impulse was given to the mounting (a single stir may impart several distinct impulses, whose temporal separations are unrelated to the natural period of the mounting); and on the further fact that at least one component of the reaction of the mounting is left unrecorded. Such being the case, an attempt to convert the magnitude of the displacement of the recording pen into an expression of the energy expended by the subject in performing the change of bodily position, is absurd.

Some displacement must therefore be chosen as the minimum to be considered. Inasmuch as instruments vary in sensitivity and in magnification, it should first be decided how much of a bodily stir is worth considering, for the purposes of the experiment, and then find the least displacement which such a stir will produce, for the subject under consideration, and treat such a displacement as the smallest which will be taken into account. It is easy enough to make an apparatus which will record the tremor of an eye-lid: Szymansky (38) made a heavy platform, hung from an overhanging beam, with brushes dragging on the floor to dampen its oscillations; it carried an easy-chair on which the subject sat. The whole system would seem to be clumsy enough, but by employing pneumatic transmission and a sensitive tambour, he found that it registered the respiratory changes in blood-pressure as well as changes of bodily position, so that he had to reduce its sensitivity to get a record which he cared to interpret. Most of the continental investigators who have worked on human subjects seem to consider the movement of a limb to be the smallest stir which is worthy of being taken into account.

The judgment which the record permits is therefore categorical: within a given temporal extent, the patient either made a stir which exceeded the minimum displacement, or else he did not. There is

no possibility of gradation of activity except in some statistical manner. The choice of a suitable procedure presents a problem, which one of the present writers (J.) has attacked as follows:

Assemble the records made on a single subject during a considerable number of experimental nights (twenty nightly records made under comparable conditions, is somewhere near a satisfactory minimum). Divide each nightly record into convenient segments: five minutes is a convenient length, as it is about 1 per cent of a fairly typical stay in bed ($8\frac{1}{3}$ hours). Mark each segment as "active" or "passive," according as the minimum displacement is exceeded or not. Choose some one of these intervals as a "zero" interval, which is made constant for every experimental night: one may choose, for example, the interval which begins at 11:15 P.M., or (disregarding the time as shown by the clock) the interval at which the subject retired, for the zero. Give each interval a number, in order, from the right or the left of the zero-interval. Next, count the number of experimental nights in which each of these intervals was found to be "active," divide the result of the count by the total number of experimental nights, and plot the quotient against the interval-number. The result expresses the relative frequency of movement, obtained by actual count, for each interval, taken over the number of experimental nights, and therefore the relative *probability* of movement within the corresponding interval on the fictitious, average, statistical night that is best represented by the empirical data. The construct is, of course, a fiction, in the sense in which every average is a fiction, and in the same degree. It nevertheless permits certain comparisons which otherwise could not be made: namely, of *systematic* effects of drugs, or of the season, or of bedding equipment, or of daily activities, or of a change of phase of a cyclical disease or mental disorder. Its use is almost necessitated by the fact that no direct means exists of measuring a *tendency* toward motility.

When we turn to the manner in which the continental investigators handled their data after they had obtained them, we find occasion for disappointment. Szymansky (38) obtained but *one record* in the case of each of his ten subjects, five of whom were men and five women, aged twenty to thirty years, all being healthy. He kept each of them in bed for twenty-four hours, and instructed them to remain as quiet and passive as possible. He permitted "indifferent" but not "exciting" material to be read. Eight of the subjects obeyed his instructions.

From simple inspection of the records, accompanied by a rather indefinitely grounded estimate of the amplitude and the frequency of jags in the record-line, he assigned each stretch of an hour, or less, to one of four categories: namely, "absolute rest," "relative quiet," "abated motility," and "active motility." The boundaries are rather vague, especially between the second and the third. The first two classes he interprets as indicative of "sleep," the basis being the results of interrogation of the subject the next day.

We shall not discuss Szymansky's interpretation of his results in any great detail, for the flexibility and the uncertainty of his statistical procedure prohibit a definite interpretation. Moreover, we must add that our own experiments have shown that large variations in the manner of resting of an individual subject from day to day are the rule, not the exception. To indicate what variability may be expected from this cause, we may suggest that if the total number of five-minute intervals during which the patient is in bed be indicated by the symbol T , while the number of these intervals in which the patient registered one or more changes of position as great as the movement of a limb is denoted by the symbol t ; the ratio t/T , which can be regarded as an expression of relative motility, varies through a range of 50 to 80 per cent, according to the subject, from night to night. Consequently, a single night's record, such as Szymansky gives, affords no reliable representation of the patient's *habitual* manner of resting: neither does a single pair of records, such as Karger (16, 502, ref. 11) tenders, afford a reliable means of comparing the influence of an experimental variable (a drug, or a fairy-story, etc.) with a stable norm. An interpretation of such meager evidence is little more than a guess. This judgment must hold, despite the fact that both of these authors guessed very astutely, on some occasions, as we shall presently see.

On the basis of single records, and a rather free interpretation thereof, Szymansky assigns his ten adult sleepers to three typical classes. The first type, so he informs us, shows uninterrupted "absolute rest" from the time they fall asleep until after midnight. After midnight they show "absolute rest" interrupted by three or four periods of "relative quiet." Five of his ten subjects conform to this type.

A second type showed "absolute rest" during the whole night with only one brief period of relative or active motility, which

involved the satisfaction of some "physiological need" (excretion?). Three subjects were said to conform to this type.¹¹

A third type is represented by two individuals, who showed five to seven periods of "relative quiet" or of "abated motility," whose total duration exceeded that of "absolute rest." He regards such a type as abnormal. One of the two subjects had suffered from toothache, and the other, contrary to instructions, had read an exciting story for two hours just before going to sleep.

It seems to be clear that by a period of "absolute rest," Szymansky does not mean a stretch of time during which the patient makes no major changes of position whatever, but only occasional, widely separated, changes. It is not clear how often these stirs must occur to cease being called "occasional." If one called "absolute rest" unbroken only while major stirs did not appear, then none of the 90 subjects thus far employed in the Simmons investigation could be classified under Type I or Type II. One subject, an insane woman, could have been considered under Type II during some three nights when she was heavily drugged, but even those records were not typical of her habitual manner of resting. All the others would belong to Type III, with huge individual variations within that type. The difference does not rest upon differences in the sensitivities of Szymansky's instruments and ours, but in the manner of taking account of what the records show.

Szymansky was not less interested in the distribution of motility over the day than in its distribution through the night. Of the time not devoted to sleep, the eight subjects who obeyed instructions spent from 56 to 67 per cent of the day in "active motility," the average being about 62 per cent. The main periods of motility came during the forenoon and late in the afternoon, thereby reminding Szymansky of a finding of W. Stern, reported in *Ueber Psychologie der individuellen Differenzen* in 1900. At different times of the day, Stern had made his subjects tap out a rhythm in waltz time, and had plotted the tempo against the time of day. The curve resembled a wide latin "M" in that it was low at the beginning of the day, rose to a maximum during the forenoon, declined until one or two hours after the midday meal, rose to a second maximum late in the afternoon, and again declined toward evening.

¹¹ One such record (schematized) appears in 38, 200, Fig. 3, Chart II. The description, given on page 203, does not fit the record as given, and indicates that the latter is inverted, so that 12 o'clock noon appears as midnight.

We now encounter an interesting argument, which has the form of a true inference, but the conclusion appears to result from a change of name. Szymansky renames the periods of "active motility," calling them periods of a "*disposition toward labor*." He then remarks that the organism can work (expend energy in *specific* responses?) more readily when it is already disposed to work than at other times. Since by assumption the periods of frequent squirming express a disposition toward labor, it follows that the times of day when they appear are the ones during which one can labor the most economically. Considered as a model of inductive reasoning the argument is capable of much improvement, as is also the interpretation which Stern put upon his own results. Nevertheless, the measurements made by Wyatt and Fraser (reviewed in 16) on the output by half-hourly periods of women factory workers on one operation conform rather well to the expectations raised by both of these authors.

Szymansky's earlier work (36) on infants indicated a half dozen periods of "absolute rest," and as many of "active motility" in the 24-hour cycle. The differences between this distribution and that of the adult he attributes in part to the change in the metabolic rate, and partly to the increasing influence of light as early infancy is passed. The motile periods of the infant tend to merge as age progresses, until, in early childhood, it may be considered "monophasic" if the afternoon nap is omitted.

Kreidl and Herz (22) obtained one 24-hour record on each of five blind subjects, together with ten nocturnal records on one of them. They also obtained one record on each of two deaf subjects, two records on two others, four on still another, and five on one besides. They used two subjects who were both blind and deaf, obtaining ten records on one and five on another. Of this collection of data, two sets—*i.e.*, those which contained as many as ten records on one subject, would justify the kind of statistical treatment which we suggested above. We venture the hope that they will receive it before they are destroyed, for the problem is important. The authors simply give verbal descriptions of the movement-records taken singly, night by night. Their findings may be summarized in a word: the descriptions would fit the records of our most typical *normal* subjects, and they also conform well to the similar descriptions of the normal subjects employed by the authors.

These results should be considered in relation to the classical

report of Strümpell, cited by the authors. Strümpell's patient was a neurotic youth, who was said to be completely anaesthetic except for impressions made on one eye and one ear. If these channels were shielded, it is said that the patient promptly "fell asleep."

It strikes us that the diagnosis of "complete anaesthesia" to stimuli applied elsewhere has been accepted rather meekly. Was the "muscle-sense" completely lacking, for example? If it was, it is hard to see how the patient could perform enough coördinated movements to live. In *locomotor ataxia* one has a virtual suppression of the muscular sense in a restricted region: what would happen if such disorganization were general? On the other hand, if this kind of sensitivity remained, the patient would not be removed from stimulation when vision and hearing were depressed. The suspicion is growing that the greater portion of the nervous current used in effecting response may be generated in muscle-spindles and in undifferentiated tissue and free-nerve endings, by the organism's own responses. If this view is tolerably correct, the classical interpretation of Strümpell's case breaks down. It has been previously suspected, of course, that the patient was shamming, after the manner of hysterics. Kreidl and Herz have tended to confirm the interpretation that the most important stimuli to waking are applied within and upon the patient's skin. Once they are effective, ordinary lights and sounds may interfere with a return to sleep; but taken by themselves they are not important disturbers.

Guttmann (12) obtained over 100 separate records, of which about half were yielded by manic-depressives. A part of his problem consisted in ascertaining what soporific agents were indicated, and when. He remarks, judiciously, that both their administration and the evaluation of their effects has rested mainly on guess-work hitherto, and that the motility-record offers a possible means of forming a more reliable judgment. One would think that the first necessary step is to determine what is the normal manner of resting of each individual patient. As was mentioned above, a person in robust health varies markedly from night to night with respect to his average. The effect of a soporific, therefore, cannot well be evaluated unless a considerable number of comparison-records are available, unless it produces a huge deviation from the normal which can be shown to be relatively improbable by the application of the laws of chance. Furthermore, it has developed in the Simmons Investigation (to be elaborated upon in a forthcoming monograph) that each individual has his own norm. The fraction of the time spent in bed

which a healthy individual spends in immotility varies from person to person through a range of more than four to one. It is therefore unsafe to assume that a given person should habitually spend any particular proportion of his time in bed, in quietude. Some such assumption appears to underlie Guttman's procedure. Certainly such records as Nos. 9 and 10c (p. 318) and 14a (p. 323) show no greater frequency of movement than the most typical records of some of our most representative subjects in perfect health: yet it was considered that veronal and luminal were indicated in the first case, veronal in the second, and tincture of opium in the third. Let us hope that the basis of such an interpretation lay outside the motility-records for none appears there.

Guttman, as well as Karger (19b) has produced some very suggestive evidence concerning the action of certain hypnotic agents. The influence of veronal and of luminal seems to be much more pronounced on the second night following its administration than on the first. The growing tendency toward household use of derivatives of barbituric acid would seem to warrant an inquiry into the persistence of the depressive effect through the next two working days, as well as through the two nights following the dose. If the persistence which these investigators appear to have discovered should prove to be general, it would seem unnecessary to administer such drugs as often as once in 24 hours, in order to obtain the sedative effect. Chloral hydrate (19b) appears to have increased motility on the first night following its administration; sodium bromide seems to have been promptly effective in the case of one child who had proved resistant to chloral hydrate and luminal. The combination of opium with veronal (12) was followed by complete immotility which endured for several hours: a condition, let us add, which almost never appears in the healthy subject in ordinary circumstances.

The time would now appear to be ripe for some intensive, well controlled experiments on the problems of soporifics which these experimenters have so well set. The method appears to be adequate if the results are properly scored and treated, and if a sufficient mass of them is accumulated to permit chance variations to cancel each other.

Szymansky (38) also required 16 subjects to spend 12 to 30 minutes in an easy chair, mounted on a yielding platform¹² to which

¹² This apparatus, though simpler than that of Renshaw and Weiss 33-a is capable of indefinitely greater sensitivity than the latter, and has much to commend it.

a recording instrument was attached. He instructed them to assume a comfortable bodily position, and to refrain from bodily movement as far as they possibly could, for the duration of the experiment. Fifteen of these subjects stirred, in the average, once in 1.2 minutes, the range of variation lying between 0.6 and 3 minutes. An additional subject appears to belong to a different class: she made but three stirs in the whole 30 minutes. If her record be averaged with the other 15, the average length of the "rest-period" for the whole group is 1.8 minutes. This is about the same average as a group of tubercular women, observed during the Simmons investigation, showed during the hourly periods in the forenoon and the afternoon when they were forbidden to sit up in bed, to talk, read, write or primp. During the remainder of the day they were still more active.

Szymansky reasoned that his subjects, in order to refrain from stirring, had to exercise volition: that the longer they maintained this form of self-control, the more will-power they had to exercise. He suggested that the measure of performance in this experimental task, under the experimental instructions, might be taken as a measure of patience, persistence, or will-power. Aside from the fact that different people may be differently irritated by stimuli which arise from a long maintenance of bodily position, a still more important question ought to be considered. Granting, as we must, that this experiment yields a direct measure (in minutes) of the subject's persistence in sitting still despite the presence of an unmeasured and unanalyzed complex of irritation, does it therefore enable us to predict how long he will persist in some other kind of performance, despite the presence of other kinds of irritation? If so, how was the fact established? By independent measurements of *both* performances, or by the Method of Proclamation—the reliance of the magician¹³ of the past and the mental tester of today?

Van Leeuwen (27) studied the nocturnal activity of a dog under four conditions: namely, normal, dosed with caffeine, dosed with extract of ordinary coffee, and dosed with extract of coffee from which nearly all the caffeine had been removed. His apparatus was somewhat like one of Szymansky's: the animal's cage was suspended on helical springs, with a rigid connection to a corrugated rubber tube, closed at one end, and connected at the other with a Marey capsule,

¹³ For an interesting example, cf. the 'test' of guilt to be applied to a wife whose husband, without evidence and without confession, accuses her of adultery, in the *Law of Jealousy*, Numbers v.

the rubber diaphragm of which supported a writing pen of a slow moving kymograph. By means of a ratchet-pawl device, the vertical displacements of the cage were made to actuate a rotation-counter. Thus the total displacement which occurred within a given time could be obtained by comparison of the two readings. The author presents some sample kymographic records and a table of differential readings on the integrator just described: one reading for each night. Because of the manner in which the results have been exploited in propaganda for decaffeinated coffee, they invite special attention here.

The dog weighed 6 kilograms (=13.2 pounds), which is roughly one-twelfth the weight of an average man. Hence, if the doses here employed are multiplied by 12, the product will express the equivalent dose relative to body weight for a man who weighs 72 kilograms, or 159 pounds.

On the nights in which no doses were given, the rotation-counter gave a maximum reading of 48, a minimum of 13, and an average of 30.5 revolutions. On a night following the administration of the extract of 16 grams of ground coffee-bean the number of rotations registered was 98.¹⁴ This dose, which, according to the author, is the smallest one whose effect can be reliably shown, is the equivalent of eight cups of strong coffee for a man weighing 72 kilograms, or 159 pounds. (The author considers 25 grams of ground bean to the cup as being strong.) This record is compared with a so-called normal record, in which the displacements of the cage produced but 31 rotations of the integrator. On their face these two records make good propaganda, provided one assumes to begin with that the dog ought to spend 14 hours of the night in resting as quietly as possible. The records made on the kymograph tend to abate the impression produced by the other readings, by giving additional information. On the night following the administration of the coffee extract, the dog showed a half hour of almost continuous locomotor activity between 5:45 and 6:15 P.M., with occasional revivals for five minutes or less at a stretch until 9:45 P.M. These periods contained nearly all the excess of activity over the normal night, although there are two other brief periods of activity: one about 12:20 A.M., and the other about 6:30 A.M. Both of the latter were followed by rest which appears to be about as quiet as that shown in the "normal" record. Whatever injurious effects may have resulted from this huge dose,

¹⁴ On another similar experimental night the same dose was followed by a reading of 70, a reduction of some 29 per cent.

none appear in the dog's motility record. In fact, the latter shows nothing save an effect which people often drink coffee in the hope of getting: namely, an evening of heightened activity, followed by a night of quiet rest.

Van Leeuwen also shows a kymographic record following the administration of the extract of 80 grams decaffeinated coffee. The integrator registered but 55 rotations: and yet, between the hours of midnight and 6 A.M. there appears much more activity, both postural and locomotor, than in the normal record or in the record which followed ingestion of the extract of 16 grams of coffee.

In a propaganda booklet (28) distributed by the vendors of one brand of caffeine-free coffee, we find some of the foregoing results, along with others in the same article (27), which is specifically mentioned; but the results are expressed only in terms of the total number of rotations of the integrating wheel, thus: Normal, 48; after 25 g. coffee, 148; after 80 g. caffeine-free coffee, 55. The figures are taken from van Leeuwen's table, but in his original report he does not give the kymographic records for the first two experiments just mentioned. We have discussed the record belonging to the third. If the distribution of activity according to the time of night be taken into account, it is extremely difficult to see how such data as these can lend logical support to an argument in favor of caffeine-free coffee.

Here we must mention that on page 7 of 28 there appears a drawing of van Leeuwen's apparatus beneath the following legend: *Verscheuchung des Schlafes durch coffein. Versuch von Prof. Dr. W. Storm van Leeuwen (Universität zu Leiden). Studien über die Wirkung von Coffein, Kaffee und coffeinfreiem Kaffee.* Two curves are shown, which resemble kymographic tracings. One is labelled *Normal*: the other, *Diese automatisch registrierte Kurve zeigt die nächtliche Bewegungsunruhe des Hundes nach Coffein.* Neither curve resembles any portion of the ones which van Leeuwen publishes in 27, and neither, in our opinion, could have been made with the recording instrument which he used.

More recently another manufacturer has reproduced (39) portions of these two charts, again referring to van Leeuwen's report, but omitting the picture of the dog and the apparatus, and giving no hint that the records and the doses pertained to a 13-pound dog rather than to a typical human patient, for whom the preparation was recom-

mended. If Dr. van Leeuwen is not a party to this deceit, we suggest that a disclaimer from him would be appropriate.

To return to the original report (27). A kymographic record is also shown of a night's activity following the ingestion of 0.2 grams caffein sodium benzoate (*ca.* 1 gram caffein). According to van Leeuwen's citation of Lehmann, this quantity of caffein represents the quantity extracted in ordinary cooking from about 16 grams of coffee. Taking 25 grams of coffee bean as representing one strong cup, this dose represents the equivalent, relative to body weight, of eight cups of strong coffee for a 72 kg. man. The reading on the rotation-counter is not given, but the kymographic record shows an abnormal amount of locomotor activity until about 12:30 A.M., followed by rest which is distinctly more quiet than that shown in the "normal" sample-record, and which remains so until after 8 A.M. We do not presume to prescribe the hours at which a dog ought to take his rest, but if only such results as are shown are to be considered, and if they are applicable to the normal human subject, this exhibit could serve as a basis of propaganda against the removal of caffein from coffee: the dog, under this relatively huge dose, did exactly what normal people hope to do after drinking coffee in the evening.¹⁵

Certain other interesting comparisons appear in van Leeuwen's table. The integrated activity, indicated by the reading on the rotation-counter, does not increase regularly with the dose. The extract of 50 grams of coffee was followed by 112 rotations: the extract of 25 grams, administered two nights later, was followed by 148, or 32 per cent more than that which followed a dose twice its size. The most quiet night reported followed the administration of 6 mg. caffein by mouth; only 12 rotations occurred. Relative to body-weight, this dose would represent the caffein-content of half a cup of strong coffee administered to a 159 pound man. On the whole, we suspect that the results of this experiment do not indicate as much of an effect of coffee and of caffein on the sleep of the human as they actually have. The assumption is implicit that the effect on the dog indicates the effect on man: as far as the motility-records go, no evil effects are shown.

¹⁵ We hold no brief for the use of coffee; neither have we any fault to find with either of these advertised preparations, as such. We protest, however, that van Leeuwen's findings are irrelevant to the propaganda based ostensibly on them. No light whatever is shed on the relation between coffee and rest by the results cited.

3. Studies which employed measurement of oxygen-consumption

Laird and Wheeler (24) describe a method, invented by themselves, of measuring what they call "mental efficiency in the true sense." They applied it to the effect of curtailing the subjects' stay in bed from eight hours to six, accomplished by postponing the time of going to bed. The logical foundation of the method requires consideration before the results.

On awakening in the morning, and while still lying in bed, the subject is connected, by means of a modified gas-mask and tubing, to a gas-receiving apparatus. If no accidents, such as leakages, occur, analysis of the collected air permits determination of the quantity Q of oxygen (measured in cubic centimeters or in grams) which the patient consumed during the test. If the weight or volume be multiplied by the proper factor (which happens not to be perfectly constant for all conditions), Q may be expressed by its heat-equivalent, in calories; and subject to certain fairly plausible assumptions, it will represent the energy converted into heat in the course of the bodily activities during the time in question. If the time T required to collect the sample be measured in hours, the ratio Q/T expresses the oxygen-consumption in (say) grams per hour, or the rate of heat-production in calories per hour during the time T . This is the temporal rate of output for the total body surface: if the latter varies from time to time, as it may, or from person to person, as it does, it should be divided into Q/T to reduce the comparisons to a common basis. In the present experiment, this was not done, and was perhaps unnecessary as one person was not compared with another. Since this determination of Q/T was made while the patient was lying still, and under instructions to remain idle, it is called a "resting" rate of metabolism; and being taken as *standard* for the day, will hereafter be denoted by Q_0/T .

During the remainder of the experiment the patient continues to lie in bed, but he is now given a list of problems in multiplication which he is to solve "mentally." After ten minutes of such work he is again connected with the respiration-apparatus, and another sample of air is taken during five minutes of continued multiplication. The rate of "working" metabolism may be derived from this analysis, and expressed simply by Q/T .

The expression $\frac{Q - Q_0}{T}$ gives, in itself, merely a comparison

of the rates at which the subject takes oxygen from the air (or at which he generates heat) at two different times. The authors put an interpretation on it which we shall consider later, but we shall first dispose of the remaining formal considerations.

The subject is required to solve n problems at each experimental sitting: in the present case $n=15$. The time T which he spent in solving them was measured in seconds, and may conveniently be expressed in hours. The expression n/T gives the rate of multiplication: if errors are to be considered, n in the expression may be reduced proportionally to the number of errors, or T increased by an amount proportional to the time consumed in making them. This correction, however, is a detail.

Let us now define a formal relationship:

$$R \equiv \frac{kn/T}{(Q - Q_0)/T}$$

which instantly reduces to

$$R \equiv \frac{kn}{Q - Q_0}$$

The factor k is a proportionality-constant, whose value depends on the units employed for measuring Q and T respectively. If one value be assigned to k , then R expresses *problems per liter*; if an alternative value is assigned, R expresses *problems per calory* (of generated heat). The expression implies *no physical relationship whatever* between problems and calories. This intrinsic lack of meaning must be remembered.

The value of R may be determined under two sets of conditions 1 and 2: an absolute comparison is given by $R_1 - R_2$ and a relative comparison by $(R_1 - R_2)/R_1$. In the present experiment, condition 1 is especially distinguished by the fact that it was preceded by a stay of eight hours in bed; condition 2 by a stay of only six hours. Each condition was studied for one week, condition 1 being taken first. Three subjects were employed, all of whom had undergone "several weeks" of practice in the arithmetic operation, and were showing "no further improvement."¹⁰

The authors do not present a formal statement of the values for

¹⁰ All the subjects, nevertheless, did give a higher speed in multiplication during the second week of the experiment than during the first, the increase varying between 0.9 and 9 per cent. The authors disregard the possibility of the effect being due to practice; in one place they suggest that it is related

the three subjects of the difference $R_1 - R_2$, but they present the data which would enable such a statement to be made, and also say, in effect, that such a comparison constitutes the crux of their study. We shall now mention their results briefly, and then inquire into their meaning.

For all the subjects R_2 is less than R_1 . Is the difference fairly large when compared with its own probable error? The authors give no measure of variability, so we cannot tell.

Under condition 2, n/T is greater, for every subject, than under condition 1. Each patient multiplied a little faster (by 0.9 to 9 per cent) after an allowance of only six hours for sleeping than after an allowance of eight. *Are these differences statistically reliable?* The authors do not tell us: neither do they offer us a means of judging, as they should have done whether they expressed an opinion or not.

Under condition 2, in the case of two of the subjects, Q_o/T is greater than under condition 1: i.e., the patient generates heat more rapidly after six hours devoted to rest than after eight hours. Probably, as the authors suggest, his muscular tension is higher, on the whole; he is perhaps generally more irritable, more responsive. Such a result is not surprising: but *how does this difference compare with fluctuations from day to day under either condition singly?* We are not informed, although the information would have been interesting whether the authors emphasized the difference or not.

Under both conditions, Q/T is greater than Q_o/T .¹⁷ That is to say, the patient generates heat more rapidly when he is multiplying than while he is "idle."¹⁸ The difference $\frac{Q}{T} - \frac{Q_o}{T}$ is greater

to the loss of sleep, and in another place deny that loss of sleep affected the "mental output." The largest improvement (9 per cent) ought to be explained somehow.

¹⁷ The authors *assume* that the difference was *necessarily incidental* to the performance of the mental work. They do not mention the earlier study of Benedict and Carpenter, done on 22 subjects, which yielded no differences that the experimenters were willing to attribute to mental work (taking examinations). This result (2) has been considered authoritative. A discussion, or at least mention, of this apparently negative finding would have been proper.

¹⁸ On some days of the experiment, a second set of readings should have been taken without the subject being required to engage in mental multiplication. Such a control would have given an indication of the effect on rate of oxygen-consumption of activities that are incidental to waking up.

under condition 2 than under condition 1: *i.e.*, although the patient is multiplying somewhat more rapidly, he is also generating heat more rapidly, after six hours of rest than after eight, in comparison with the standard adopted for the day. *What is the statistical reliability of this difference?* This information, also, is withheld.

We must now inspect the authors' interpretation of the relationship between problems solved and heat generated which we have denoted by R . The empirical finding that Q/T is greater than Q_0/T means simply that energy was converted into heat more rapidly *while* the patient was doing mental multiplication than while he was not. The authors assert that *all* of this additional expenditure was invested in activities which were *necessary* to the performance of mental multiplication: "A comparison of the working and resting caloric consumption shows how much additional energy is needed to do the mental work." Thus, by assumption, $Q - Q_0$ measures the energy-input of mental work, and n the mental output: whence, $R \equiv kn/(Q - Q_0)$ is a perfect expression of "*mental efficiency in the true sense*," and is shown by experiment to be greater after eight hours of rest than after six.¹⁹

What the authors have done to establish their "true statement of efficiency," is to give a new name to $Q - Q_0$, and then to reason as if their christening had miraculously endowed it with the properties which are suggested by the name. They now call it an expression of energy expended *in* multiplication, instead of additional energy expended *during* multiplication, disregarding the fact that multiplication was accompanied by other activities, not all of which are known to be necessary thereto, which also call for the expenditure of energy in *variable* amounts and rates.

Their interpretation implies that two things can be, and have been, determined: namely (a) what members of the universe of bodily activities occurring at the same time are necessary to mental multiplication, what activities are irrelevant to it or dissociated from it, and what activities are interfering with it; and (b) at what rate energy is being expended in each of these three classes of activity. If this review were intended solely for the technical reader, further comment would be unnecessary; but for the benefit of the non-technical readers, cultural and general, to whom the original report was addressed before it was submitted to the scientific public for

¹⁹ *At the time of day at which the test was made*, of course, and subject to the conditions, described and undescribed, which then held.

inspection and criticism, we may add that Laird and Wheeler made no such determinations; and that the technical methods which are now available in physiology and biophysics are too limited to make such determinations possible. What they tender is a counterfeit method of attacking the problem of "*What it costs to lose sleep*," along with a sham solution, neither of which need to be considered further.

4. *Studies which employed histological examination of tissues.*

Bast and his collaborators deprived rabbits of sleep by placing them in slowly rotating cages, which compelled the animal to take at least one step as often as eight times a minute.²⁰ Thus the animal had to walk, at the least, 1.4 miles a day, but this minimum requirement was not considered excessive. Satisfactory observations were made on 14 animals out of the total number studied. Nine out of the 14 developed characteristic symptoms before death. The other five were killed on the 30th day, without showing characteristic symptoms. Collapse was typically preceded by a sudden fall in temperature, a considerable rise in pulse-rate above its recent level, followed by a sudden fall, and a gradual decline in the rate of breathing. One animal appeared to have attained complete exhaustion at the end of seven days, and another at the end of 17 days; but after five or ten minutes in which there were some convulsive movements, they resumed their former activity without further evidence of fatigue. Only four animals attained complete exhaustion while the observer was present: they showed the following symptoms: body relaxed, but muscles showing fibrillary contractions; eyes half open; corneal reflex absent; marked salivation; sphincters relaxed; average pulse-rate 132 per minute; average respiratory rate 42 per minute; rectal temperature 35.6°C. (= 105.1°F.); handling the animal while in this condition caused convulsions.

Following this seizure, the body completely relaxed, the pulse became irregular and feeble, the temperature dropped rapidly, and the respirations stopped. Heart-beats continued for one to two minutes thereafter. It was impossible to predict with certainty the

²⁰ There appears to be an important difference between the treatment of the animals in this experiment and in that of Crile. In this experiment, the disturbances appear to have been regular, so that the animals had opportunity to learn to take their rest in temporal segments of a few seconds each. It may be that some of them succeeded in making so profound a modification of their normal habits. In Crile's experiments it appears that no such opportunity was given, for the animals were disturbed whenever they slumped down to rest.

onset of complete collapse: for the characteristic symptoms were in many cases repeated several times, to be followed by recovery of temperature, pulse-rate and respiratory rate, which might be retained for several days.

Nervous tissue from sixteen fatigued animals and seven normal animals was subjected to histological study. The technique of preparation was very carefully controlled to avoid spurious effects, such as have been suggested as a possibility in the work of previous authors. The point is emphasized that by ignoring the great variety of appearances of tissue in supposedly normal animals, one may easily draw erroneous conclusions from comparison of experimental animal tissue with that of normal animals.

Their examination of nervous tissue partially confirmed the findings of Crile (4), Hodge and Dolley (cited in 4), all of whom had reported evidence of nerve-cell degeneration in exhausted animals. The present authors found cells in the gray matter, usually in the visceral motor area, which showed partial or complete chromatolysis, although most of the cells, particularly in the somatic area, appeared normal. This was true only of animals which had been carried to complete exhaustion: in others, no deviations from the normal were detected.

Their criterion of chromatolysis was two-fold: granulation of Nissl's bodies, and vacuolization of the cytoplasm. They regarded all other symptoms as unreliable. They observed no shrinkage of the cells or the nucleus, or migration of the nucleus toward the periphery. The nuclei of the cells in the normal animals took a light stain: those of the exhausted animals did not.

Chromatolysis was found in some nerve-cells of normal animals: hence it was only because a definite nuclear change, as well as chromatolysis, was found in the exhausted animals that the authors were willing to attribute degenerative changes to fatigue. Ruptured cell-walls were sometimes found in the vagus. In the medulla, chromatolysis was variable. Characteristically it seemed to begin at the cell wall and progress inward toward the nucleus. In any given area, most of the cells were usually normal in appearance, others being moderately changed and still others almost completely colorless. However, some fields were found in which most of the cells refused to take the stain. Some degeneration was found in the spinal cord, but less than in the medulla.

While their findings, on the whole, were positive, they remark

that "it is not as easy to detect nerve-cell changes following prolonged periods of sleeplessness as one might be led to expect judging from the findings of those who have studied the effect of other forms of fatigue on nerve cells."

5. Theoretical contributions

Carlson (3) emphasizes the importance of muscular relaxation in the production of sleep, suggesting, as one of the present authors (*J*) had done, that muscular tension may provide the principal source of stimulation necessary to normal activity. The same suggestion is proffered by Dunlap²¹(9), who stresses the disintegration of activities in drowsiness and sleep. Hollingworth (13) treats sleep as a silly, vicious habit, more devastating than alcoholism, opium-addiction, and yellow fever, for which no excuse can be found except that everyone indulges in it. His general viewpoint appears to be essentially that which was held, for a time, by such religious leaders as St. Francis of Assisi, St. Teresa, and the Methodist leaders Fletcher and Wesley; all of whom endeavored also to practice it, but abandoned it in the face of the results of the attempt.

6. Miscellaneous Studies

The work of Landis (26) shows that changes in the electrical resistance of the body are apparently unrelated to sleep and waking, or to "mental activity" while the subject is awake. He attributes the results found by Richter (16, 502, ref. 20) to changes in the conditions of the electrodes (which occur without the patient's body being included in the circuit), to variations in intimacy of contact, and to a counter electromotive force set up in the body in opposition to the polarizing current. Only one-fifth of the total obstruction offered to the latter is found in the skin;—not all, as Richter had claimed. The work of Darrow (7) indicates that Landis is partly wrong: that the varying resistance to the external current is probably due, almost altogether, to the activity of the sweat-glands. This interpretation is strengthened by the proof, by means of Hathaway's apparatus, that the so-called psychogalvanic reflex can be obtained by the use of alternating current.

²¹ The hypothesis of Johnson (18) arose, not from this article, but from acceptance of the doctrine of homeodetic activity set forth in Dunlap's treatment of *Images and Ideas*, published in the *Johns Hopkins University Circular*, 1914, Whole No. 263. The essentials of this doctrine are also implied in Herrick's notion of functional integration, and were expressed still earlier by James.

Because of limitations of space, we are compelled to postpone a general review of studies on learning and forgetting as related to sleep.

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GENETIC STUDIES OF EMOTIONS

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I. METHODS

1. *Individual case studies*

Recent years have witnessed the accumulation of vast numbers of diary observations, clinical records, and psychoanalytic histories. In the present review no attempt has been made to sift this literature for reports on emotions, nor to duplicate surveys covering the topics of mental hygiene, psychopathology or delinquency. Much of this material is of suggestive value, but as a research method the various types of personality studies are not as a rule sufficiently quantitative to provide more than a general background for data collected through other procedures.

2. *Questionnaires*

Two sorts of questionnaires have been used in connection with problems of emotional development: (a) with questions concerning observed behavior in others, (b) with questions concerning the subject's own present or past experience of emotion. The first of these is illustrated by Hall's classical studies of laughter (49) (1897), and of anger (50), which contained such questions as the following: "Recall a few cases of great laughter in children and describe its cause." "Describe overt acts (involving anger): describe every vasomotor symptom, . . . all changes in muscle tension . . . In description be photographically objective, exact, minute and copious in detail." The second type of questionnaire is illustrated by Hall's study of fear (48), in which he circularized 1,700 teachers and school children, requesting information concerning fear tendencies and remembered fear episodes. Evidence obtained in this manner suffers of course from sampling errors and from intrinsic difficulties in following instructions.

The earliest systematic questionnaire covering emotional traits, with especial reference to emotional stability, is the Personal Data sheet devised by Woodworth. With adequate norms and with standardized procedures, a questionnaire of this type becomes a "test." B. Johnson (55) employed an adaptation of the P.D. sheet,

in a study of malnourished as compared with normal children. Mathews (87) constructed a special form of the questionnaire for use with school children; 70 items were included, dealing with fears, worries, unsocial moods, etc.; the reliability was reported as .67, but in a tentative validation through the comparison of problem and normal children, the test failed to demarcate the two groups. E. G. and C. W. Flemming (39) have checked the Mathews Revision against teachers' estimates of emotional balance, finding small negative correlations which were unaffected by partialing out age or intelligence.

A questionnaire in the form of a standardized oral interview, is illustrated by the study of Town (130), who described a series of 80 imaginary situations to the subject, with the request that he indicate "how he would feel or what he would do." The (imagined) situations are considered to be provocative of the fundamental emotions listed by McDougall (fear, disgust, curiosity, anger, elation, subjection, tenderness, sociability). Goodenough (45) has reported an interest questionnaire, with particular reference to the question, "Suppose that a fairy were to grant you three wishes. What would your wishes be?" The results were regarded as of practical value in clinical procedure, for subjects having an M.A. of 9 or over.

The expounders of questionnaires have not been idle in Europe; in fact the method has emerged in places as a respectable branch of "experimental reflexology." This is the case, for example, in the work of Shevaleva and Ergolska (116), which involved a no more rigorous technique than the classification of children in rotating observation groups as "relatively non-excitable, consistently excitable, or unstably excitable." Studencki (126) has employed a questionnaire in studying "children's relations to themselves," and Baumgarten (7) has utilized the unsettled social conditions following the German invasion of Poland, to obtain questionnaire data from children concerning emotional attitudes associated with "hate." Hatreds arising within family groups have been investigated by Robin (109), in a study of French families.

While questionnaire methods have been appraised as belonging to the "underworld of science," they still appear to retain some usefulness in opening new areas for investigation. From standpoints which emphasize the un verbalized character of emotion (Watson, 134), questionnaires would be expected to achieve a lower validity in the field of emotion than in other branches of psychological research.

3. *Trait ratings of children*

One of the earliest uses of ratings in a genetic study was that of Pearson (96) (1904). Ratings of siblings by teachers or members of families were obtained for such traits as "vivacity," "assertiveness," "temper," etc. Data were collected on about 2,000 pairs, but the ratings were roughly made on only a two or three-point basis. Heymans and Wiersma (52) obtained ratings from 3,000 physicians on parents and children, with regard to emotional and other traits; this material was later used in a study of family resemblance by Schuster and Elderton (110), but has frequently been criticised on the ground of non-comparability and the probability of excessive chance errors.

A more careful technique was employed by Terman and Goodenough (128) (1925) in their study of trait ratings of gifted children. A graphic 7-point scale was used, with concrete definitions of each step on the scale, and with the midpoint of the scale serving as "average for age." However, in the case of emotional traits the correlation of parents' with teachers' judgments was so low as to be negligible. In their discussion of mood type, mood stability, and emotional response type and emotional stability, S. and M. G. Blanton (14) have described a somewhat different form of rating scale, based on departures toward either extreme from an "ideal average." The incorporation of ratings into a "mood chart," with data on the influence of emotional disturbance upon later mood fluctuations, has been reported by M. C. Jones (66).

L. Marston (86) has devised an Introversion-Extroversion rating scale, consisting of 20 alternative items, with a provision for a graded estimation of each pair of items; the form of the scale results in a bimodal distribution of ratings. By the split half method, reliabilities of from .83 to .98 were found, while different raters agreed to the extent of an average r of .71. Powers (100) has used this scale with adolescents, reporting results very similar to those obtained by Marston with preschool children.

In a rating chart using teachers' own wordings about behavior, Bridges (20) arranged fifty items in the form of paired opposites, including such emotional traits as "venturesome or timid," "not excitable or excitable," "spontaneous or restrained." On a small nursery school group, ratings were found to correlate nearly perfectly with order of merit ranking of the same children.

Moore has devised an infant rating scale, which was applied by

Bonham and Sargent (18) to 120 children, rated, within two weeks of birth, by nurses in a maternity ward. Thirty-five of these were followed up by ratings on the Bonham-Sargent scale at 24 and 30 months. A number of emotional traits were considered, and correlations studied for traits at successive ages.

In a review of rating methods, the writer has commented, (58) "It is doubtful if ratings, taken by themselves, can contribute anything of fundamental importance in the study of child development. Their value lies in yielding certain background information, of a greater degree of compactness and (apparent) definiteness than the data in diary records." Granting that the validation of other procedures rests eventually upon some form of rating, it nevertheless appears to be true in psychology as in other sciences that our fundamental gains lie in the direction of more objective methods in measurement.

As Goodenough and Leahy (47) have shown, ratings may have a definite value in the differentiation of groups. They obtained significant results in a comparison of birth orders on the basis of teachers' ratings. The legitimacy of this method rests, of course, upon the uniformity of the procedures used by different raters, the proper distribution of the groups to be compared, and in certain cases, upon keeping the raters in ignorance of the particular problem which is being studied.

4. Inventories of child behavior

The difference between questionnaires, ratings and inventories is often merely nominal. A rating may be regarded as a more quantitative type of questionnaire, while an inventory attempts a systematic survey of objectively noted characteristics.

The Andrus inventory (5) contains many items similar to those appearing in the Woodworth P. D. sheet, but is checked and scored on the basis of observers' records of behavior. In the hands of a trained person, the instrument is claimed to be "probably as objective as the usual mental test." Norms, however, are not regarded as feasible, and the value of the inventory is stated as ancillary to ratings, and to an understanding of a child's relative development in the several groups of functions represented in the inventory.

Yepsen's (141) personal-behavior score-card is another example of an inventory, designed to appraise maturity of development with reference to a large number of emotional and other traits.

5. *The method of repeated short samples*

Olson (95) and Goodenough (46) have applied a method involving twenty or more behavior observations, covering a period of from one to five minutes. The scoring for each period is based on the presence or absence of certain specific and objectively defined forms of behavior. Olson obtained reliability coefficients for single age groups of from .4 to over .8, in the measurement of such nervous habits as thumb-sucking, nail-biting, etc. Goodenough found that on such a trait as "anger," two observers (not working simultaneously) would obtain scores correlating to the extent of .7. An increased number of periods was found to increase reliability in agreement with the Spearman-Brown prediction—a significant finding from the standpoint of controlled measurement.

6. *Paper-and-pencil tests*

Elonen and Woodrow (33) have employed a free association test as an indication of psychopathic tendencies in children. With sixth grade children a reliability of .86 was obtained, and a correlation of .57 between pathological association scores and composite ratings of behavior. An oral free association technique would no doubt be applicable in lower age ranges; in children with limited vocabularies it remains to be determined whether an idiosyncratic association would be more or less significant than in older children.

Furfey (41), attempting a measure of an assumed variable which he terms developmental age, has devised a test with norms down to the age of eight years. Chambers (29), working with the Pressey X-O test of the emotions (101), has established a differential score which correlates $-.12$ with intelligence, but shows an increase in medians from grade to grade. Tjäden (129), using the Pressy X-O with a group of delinquents of high I.Q., found that it failed to discriminate them emotionally from Pressey's original sample of college students. It was stated, however, that the tests were valuable "in revealing constellations of ideas which have marked emotional content." Tests of this nature, including also the Colgate Mental Hygiene inventories and the Downey Will-Temperament profile, are of course chiefly adaptable for use with adolescents and with adults. Raubenheimer (103) has reported considerable success in the use of a series of discriminatory tests with young delinquents. In this, however, we are approaching the field of character tests, which has been elsewhere very adequately reviewed.

7. Tests of emotional responses in laboratory situations.

The testing of pain sensitivity by the earlier child biographers and physiologists, was conducted chiefly from the standpoint of problems in sensory psychology, rather than with reference to problems in the emotions. The schedule of provocative situations used by Watson and Morgan (137) (1917) in their study of love, anger and fear represented an important departure in the direction of a laboratory study of the emotions. A later description of these situations, with photographs, may be found in Watson (136).

L. Marston (86) (1925) devised a series of experimental situations for the study of social resistance, compliance, caution, degree of interest, and self-assertion. The child's behavior in a standardized situation is scored with reference to 5 or 6 defined degrees of "extroversion." Intercorrelations of the various tests ranged from .04 to .61. The average correlation with introversion-extroversion ratings was of the order of .4. Marston gives a useful bibliography of 127 titles.

H. E. and M. C. Jones (60) have reported a series of laboratory tests of the response to animals, conducted over an age range of from fourteen months to twenty years.

C. Buhler (22), working with over 100 children up to 22 months of age, studied reactions of domination and submission by placing two children together with a single toy.

Meyer (89) conducted an experiment with 30 nursery school children, involving proximity to a guinea pig which was at large in a laboratory room. Ratings of eight different phases of reaction were made at several points during the experiment.

Reynolds (105) has attempted to test negativism in preschool children through a series of standardized situations, the most important of which were called "surrender" and "imitation." Fairly reliable scores of negativism were reported, which were found to be independent of I.Q. and educational background, although inversely related to C.A. and M.A. The decrease of negativism, in children from twelve to forty-eight months, has also been studied by Zaluzhni (142).

May's (88) studies of children's responses to a variety of disturbing or distracting stimuli may also be classified here, although his test batteries were primarily concerned with measuring the inhibition of overt movement.

Laboratory studies give considerable promise of adding to

our knowledge of emotional development, particularly when they are conducted cumulatively on the same children, and with due regard to the child's general reactions. Up to the present time, these experiments have lacked a unified program, and have suffered from a too desultory and too sporadic method in the approach to basic problems.

8. *Instrumental procedures*

Instrumental studies of children's emotions have developed very slowly, perhaps chiefly because of the modification required in the usual apparatus techniques before they can be applied in the lower ages. Volkelt (132) (1926) has reported a summary of fifteen years of European research on children. The only experimental report which appears to bear upon children's emotions, is that of Canestrini (27), who obtained kymograph curves of respiration and fontanelle pulse in the case of 70 infants, ages six to fourteen days. Unpleasant tastes produced evidence of emotional disturbance, while a milk and sugar solution was stated to have a quieting effect. It was also claimed that the curves showed a marked flattening when the mother whispered to the child, while strange voices had no effect.

A pioneer instrumental study was that of Binet and Courtier (9) (1897). In plethysmographic observations of a small number of children, eight to ten years of age, characteristic changes were noted in response to emotional stimuli (a reduction in pulse height and a fall in the volume curve).

Eng (35) has made a more extended test of this method, using a pneumograph and a plethysmograph in a comparative study of eleven children (ten to twelve years of age) and fourteen adults. Excitement was characterized by a high pulse and a high volume curve, with occasional irregularities in the respiration curve. She believed that the kymograph record revealed characteristic differences between unpleasant and pleasant emotional states, and that these could be further classified according to their origin in sensory stimuli or in "spontaneous" processes.

H. E. Jones (57) has developed a technique for galvanometric measurement in infancy, employing silver foil electrodes which are covered with kaolin paste and bandaged to the sole and calf of the leg. The psychogalvanic reactions obtained to a variety of provocative stimuli show the usual characteristics of the responses found in adults (long latent period, a typical curve form, and a quick

exhaustion of the response by repeated stimuli). The value of the instrumental registry lies in the objectivity of the records, and in the possibility of working with less intense stimuli than those employed for ordinary observational studies. His results are in opposition to the earlier negative findings of Peiper (98), in galvanometric studies of children under one year of age.

In later reports of work with nursery school children, Jones (58, 59) has described a galvanometric apparatus involving a continuous roll kymograph equipped with accessory ink pens for the registry of stimuli and of specified phases of overt behavior. Individual differences were recorded in initial resistance, resistance trends, resistance rhythms, and in the magnitude of psychogalvanic responses to a standard schedule of stimuli.

Denisova and Figurin (31) have studied the respiratory variations in infants by means of the Lehman pneumograph. While they were primarily concerned with rhythmic changes during sleep, their data on the relationship of respiratory cycles, urination, and overt motor phenomena are of great interest from the standpoint of a study of physiological factors underlying emotion and mood.

A method for registering somatic effects (or accompaniments) of emotion has been described by B. Johnson (56). Pressure applied to a stylus is recorded on a drum, and variations in muscle tension can be read with reference to a "tension base." The method is applicable with children as young as two years.

9. *The method of "co-twin control"*

The rôle of hereditary factors in emotional development has been studied by Gesell (43) from cumulative records of the emotional behavior of identical twins. Personality and temperament data on identical twins reared apart have been recorded by Müller (93) and Newman (94).

II. THE "PRIMARY EMOTIONAL PATTERNS"

The child biographers who described emotional processes in infancy were not as a rule concerned with systematic formulations; the listing of the "primary emotions" was left to more speculative writers.

In 1917 Watson and Morgan (137) contributed a list of primary emotions based upon empirical observations of infants. These were (1) fear, in response to loss of support or loud sounds, (2) anger, in response to hampering of movements, and (3) love, in response

to stroking or manipulation of some erogenous zone, or tickling, patting or gentle rocking. The shock effect of sounds had been previously discussed by Shinn, who agreed with other observers as to the great variability of the emotional reaction to auditory stimuli. Shinn (117, p. 209) has also noted the efficacy of equilibrial disturbance in producing fear, citing notes from Tiedemann, Mrs. Hall and Mrs. Moore. Watson omits mention of the emotional reaction to pain. Although in infants the pain threshold is high, there can be no question as to the ability of pain stimuli to elicit emotional responses even during the first few weeks. Sherman (112, 114), H. E. Jones (57), and Tayler-Jones (127), report infants' reactions to cutaneous pain stimuli, administered under laboratory conditions. Earlier observers (as Kroner [76], and Preyer [102]), have also made records on this point. An excellent review of the earlier literature is given by Shinn (117, pp. 32-37).

The definition of the sources of anger and fear exclusively in terms of stimuli has been challenged by a number of writers. In a study of responses to a snake, H. E. and M. C. Jones (60) have shown an absence of fear behavior under two years; from two to five, fear of snakes is positively correlated with age, even when the possibility of specific conditioning is ruled out. This has led to a definition of the native fear conditions in terms emphasizing the "preparedness" of the organism, rather than purely in terms of the objective characteristics of the stimulus. A similar view is advanced by English (36), and is of course not unfamiliar in the theoretical literature on the emotions.

Blanton's (13) study of crying during the first month of life is one of the best analyses of the stimuli to crying, and of the response patterns. Crying was elicited by a variety of noxious stimuli, by hunger, and probably also by fatigue and deprivation from exercise. Crying in the newborn, on the basis of observations of 98 infants, has been studied by Bryan (21). Watson (134) has reported on a study made by M. C. Jones concerning the common sources of crying and laughing in institutional children (sixteen months to three years). Eighty-five situations were listed, the most common sources of crying being "having to sit on the toilet, having property taken away, being left alone, working at something which won't pan out." The most common sources of laughing were "being played with, romping with other children, playing with toys, teasing other children, watching other children play."

Lippmann (82) has reported crying as showing its greatest frequency between six and one-half and ten months.

Enders (34) has also reported an observational study of laughing, with evidence for the provocative nature of sound, motion, and social situations.

The development of smiling, in response to a visually mediated social stimulus, has been described by M. C. Jones (65) in a sample of 185 cases. The youngest child to smile in a prescribed situation was thirty-six days of age; the response appeared in 100 per cent of the children of ninety days or older, with a somewhat earlier maturation in negroes than in whites.

Watson (136) has reported several incidental studies of jealousy. Berne (8) has found three-year-olds more rivalrous and more jealous than two-year-olds. Foster (40), in an investigation of 50 habit clinic children one to six years of age, compared with a control of 100 "non-jealous" children, found that the jealous child is more often a girl between three and four, and frequently the oldest child; she is subject to mild neurotic fears, and shows a tendency toward markedly extroverted behavior. The employment of control groups in certain types of clinical studies is an important step in the direction of a more adequate research use of this method.

In a study of the major emotions of mental defectives, Morrison (91) obtained data on over 200 institutional cases, 35 of whom were under ten years of age. Anger and affection were found to be frequent in all grades except the lowest idiots, and ratings on each of these correlated approximately .7 with intelligence. No relation was found between fear and intelligence.

The response patterns involved in the emotions of rage, fear and love have been described by Watson and Morgan (137), and in several later texts or semi-popular presentations by Watson (133, 134, 136). Fear involves a checking of breathing, a bodily start, crying and visceral responses. Anger involves a temporary cessation of breathing, stiffening of the body, screaming, reddening and later "bluing" of the face, and visceral responses. Love responses are indicated as involving a cessation of crying, with gurgling, cooing and visceral reactions. In a series of ingenious experiments, Sherman (112) has demonstrated the inability of observers to differentiate the emotions of infants, unless aided by knowledge of the stimulating situation. If emotions cannot be defined purely in terms of stimuli or of responses, we are forced to resort either to the con-

cept of an undifferentiated emotion of "excitement," as advanced by Stratton (123), or to a classification of the emotions based on functional relationships.

III. THE MODIFICATION OF THE EMOTIONS

The possibility of conditioning the emotions was demonstrated by Watson and Rayner (138) (1920) who conditioned an eleven months child to the sight of a white rat, through four associations with a loud clang. Evidence was given for a diffusion of the response to other animals and to various furry or hairy objects. A month after the last conditioning, the directly conditioned and the transferred emotional responses were still present to some extent.

Moss (92) conditioned two children, two and four years of age, to the sound of a snapper by the use of (a) the taste of vinegar and (b) cutaneous pain stimulation. In the first case a spreading of the conditioning was noted, to other phases of the situation.

Skerrett (118) has reported the conditioning of a seven-months infant to an olfactory stimulus. Evidence for the conditioning of visceral processes was given in a much earlier study by Bogen (17) (1907) who reported the establishment of a C-R between a sound and the flow of gastric juice, in a child of three years.

Conditioned psychogalvanic responses were first reported by H. E. Jones (57) (1927) who worked with infants from two to five months of age, associating previously indifferent visual and auditory stimuli with a mild electrical stimulus applied to the skin. The phenomena of extinctive inhibition were demonstrated (as distinct from ordinary P.G.R. exhaustion), with re-appearance of the C-R after an interval. C-Rs established by from 4 to 14 conditionings, were shown to survive at least three weeks without any form of reinforcement.

English (36) has described two instances in which a fear was transferred to an associated stimulus previously incapable of eliciting fear. In a third instance, an attempt was made to condition a child to a loud sound, but was unsuccessful because of the ineffectiveness of the primary stimulus. English believes it is not yet established that these transferred emotions can be classified as examples of "conditioning."

Blatz and Bott (16) have given illustrations of conditioning by shock and conditioning by social contagion, as contrasting with the usual laboratory method of conditioning by repeated association.

Anderson (4) has described an interesting case of the reappearance of a lapsed fear, which he attributes to reconditioning through a dream.

Watson depicts the complex emotional life of the adult as the outcome of numerous conditionings of three elementary patterns. Allport (3) has formulated these changes in greater detail, in terms of efferent and afferent modifications. On the basis of laboratory studies, however, Gesell (43) and H. E. and M. C. Jones (60) have emphasized the rôle of intrinsic maturation in the development of emotional patterns. Evidence is cited for the late appearance of specific fears which could not readily be explained in terms of conditioning.

The elimination of fears by laboratory methods has been studied by M. C. Jones (61) (1924), who reported on results from the following methods: Elimination through disuse, verbal control, negative adaptation, repression, distraction, direct conditioning, and social imitation. The latter two methods were the only ones meeting with unqualified success. Direct conditioning involved associating a fear object with a "craving-object," and replacing the fear by a positive response. A more detailed report on this method was given by the same writer in (62), with two subsequent summarizations (63, 64).

IV. PERIODICITY IN THE DEVELOPMENT OF EMOTIONS

Levy and Tulchin (80) (81), in a study of baby conference children, six to fifty-four months of age, report that in test situations the most intense and the most frequent resistant actions centered about the 18th month for girls and the 30th month for boys. Keens and Blatz (69), in a study of nursery school children, have found a greater frequency of emotional episodes in three-year-olds.

Busemann (25) has reviewed the literature on periodicity, and on the basis of a considerable number of studies has concluded that certain critical phases or periods of excitation occur at ages three, six, nine, twelve or thirteen, and sixteen or seventeen. The "emotionality crises" at three and six are evidenced chiefly by data from language studies.

In a study of misdemeanors in school, Blatz and Bott (15) have reported a striking increase of misdemeanors in boys of the eight and nine-year old groups, with a smaller peak at thirteen-fourteen.

Both Hetzer (51) and Ch. Bühler (23) have reported a prepubertal "negative" phase.

In Hall's (48) study of fears, periodicity is either absent or obscured by the characteristics of the data collection; boys' fears were said to increase from seven to fifteen years and then to decline, girls showed an increasing frequency of fears from four to eighteen.

Lehman and Witty (77, 78), in their study of play interests, have found no evidence for periodicity, and have concluded in favor of a gradual growth rather than of sudden jumps in development.

In an investigation of the emotional attitudes of 230 clinic children, through a statistical study of dreams, Blanchard (10) has failed to find the influence of any specific age factor, except that relatively few dreams are reported by children under six.

Regensberg (104) has discussed emotional maladjustment in gifted children, finding some evidence of periodicity which is environmentally determined by accession into school groups of equal intellectual level but of superior emotional maturity.

Cyclical variations in mood in infancy have been discussed by Shinn (117, pp. 213-214), without any attempt at quantitative expression.

The study of short-time as well as of long-time rhythms in emotional pattern appears a promising field for coöperative study by psychologists and physiologists.

V. EMOTIONAL "TYPES"

Rich (106) has examined blood specimens of 303 children for inorganic phosphorus and creatinine, and obtained correlations with ratings for such traits as good nature, perseverance, leadership, etc. Rich has concluded that chemical mechanisms contribute to the total personality make-up to an extent represented by correlations of from .2 to .3. It is clear that the biochemical approach cannot henceforth be overlooked in studies of emotional characteristics.

Emotional correlates of the "T-type" and the "B-type," in relation to calcium metabolism, imagery characteristics, etc., have been discussed by W. Jaensch (53). Jaensch's type theories are reviewed by Gesell (42). Hallucinatory experiences as an expression of emotional difficulties, have been discussed by Sherman and Beverly (113).

Peters (99) has made an analysis of school failures, classifying

them in three types. An undetermined type (similar to Meumann's verbal and Binet's unstable type) shows a poorly organized and purposeless excess of activity; a passive or inactive type develops slowly because of a deficiency in experience, and is similar to Binet's "arrested type." A third type comprise the "feebly inhibited."

Kretschmer's (74) type theories, which have been vigorously criticised by Kritsch (75) as a throw-back to an earlier physiognomical point of view, have received some support from Krausky (73) in a study of 100 school children; the latter claimed that four out of five conformed to the constitutional type classification. The behavior relationships to these types are not yet satisfactorily interpretable in genetic terms.

In a study of delinquents nine to eighteen years of age, Wires (140) found no profile characteristic of any type of delinquency; the composite profile of the total group showed high scores for impulsion and low scores for resistance and inhibition. The general literature on type theories has been reported by Klüver (70, 71).

VI. RELATED VARIABLES IN EMOTIONAL DEVELOPMENT

The influence of Watson in laboratory studies, and of a clinical or therapeutic point of view in field work, have led to an emphasis of environmental determinants of the emotions.

The earlier studies of hereditary factors have in recent years been limited chiefly to problems involving intelligence rather than temperamental characteristics. Pearson (96), however, has given evidence for a degree of familial resemblance in emotional traits approximating that found in physical or intellectual traits, and has interpreted this in favor of the influence of heredity.

Davenport (30) and Finlayson (38) have perhaps over-simplified the problem, by describing the inheritance of excitability in terms of a Mendelian dominant character. Levy and Patrick (79) have considered familial factors manifested in the production of children's temper tantrums and other emotional manifestations. The numerous genealogical studies which have been made on the subject of neuropathic inheritance will not be summarized here. Reference has already been made to studies of the emotional characteristics of identical twins reared apart (93, 94). The evidence is inconclusive.

Birth order as a factor in emotional development has been studied by a number of investigators. Stratton (122), employing self-ratings of the degree of fear and anger experienced in a large number of concretely described situations, reported the first-born as more subject to anger. Goodenough and Leahy (47) used teachers' ratings of 293 kindergarten children. The first-born obtained significantly lower ratings in aggressiveness and self-confidence, and higher ratings in suggestibility, seclusiveness and introversion. Stuart (125) studied deviations from normal emotional expression in 465 young men tested by the Colgate Mental Hygiene inventories; deviations were unrelated to birth position, but showed a slight increase with size of family. Busemann (26), in a study of school children, has reported an inverse relation between size of family and the occurrence of hyperactive or introverted tendencies, while Slawson (119) has given evidence for a slight but significant correlation between size of family and male juvenile delinquency. Breckenridge and Abbot (19) and Pearson (97) have found a greater incidence of delinquency or criminality in the first born.

Fenton (37) has briefly reviewed the literature concerning undesirable social traits in the only child, citing quotations from Bohannon, Hall, Mead and Abel, Wexburg, Brill, Coriat and others. The common belief that only children are deficient in emotional stability, was tested by a study of teachers' ratings of approximately 200 children in an elementary school. No significant differences were found between only and other children. Similar negative results have been reported by Stratton (122) and Stuart (125), while Blatz and Bott (15) find that only children have the fewest misdemeanors reported in school. Goodenough and Leahy (47) have obtained for only children higher ratings in aggressiveness, instability of mood, and flightiness of attention, and Burt (24) in a delinquent group has found a higher proportion of socially "only" children than in a control non-delinquent group.

The inconsistent results obtained in these investigations are no doubt to some extent due to sampling errors involved in the use of "marked cases." In further more analytic studies of the influence of birth rank, cases should be selected with reference to such factors as the relationship of family size to intellectual and social status; the relationship of pregnancy order to pre-natal and parturitive conditions, and the phenomena of social interaction in family groups of varying sizes.

The influence of birth trauma upon the development of emotional behavior has been discussed by a number of writers, of whom the most recent is Schroeder (111).

The effect of organic disturbances such as encephalitis has also received considerable attention. Koseki (72), to cite one example, has given a number of case histories of children who suffered a marked change in temperament after a cure of encephalitis lethargica: an increased assertiveness and excitability, with no accompanying changes in intellectual status.

Stratton (121) (124) has studied the relation between emotion and the incidence of disease; persons who have been subject to disease tend to respond more intensely to anger situations and probably also to fear situations. A history of disease before the age of 6 appears particularly significant in relation to later tendencies to anger. Stratton conservatively points out that the relationship may rest upon (a) a common constitutional predisposition, (b) the effect of disease in predisposing emotion, (c) the effect of emotion in predisposing disease.

Data on intellectual development as influenced by an endocrine disorder (puberty praecox) have been surveyed comprehensively by Doe-Kulmann and Stone (32). In view of the relationship between the emotions and the endocrine organs (reviewed, up to 1925, by Rikimaru (108), it would seem well worth while to make a thorough genetic study of emotional correlates in endocrine disorders.

Lippmann (83), on the basis of results from the administration of atropine, has emphasized the importance of congenital autonomic unbalance, in the production of certain types of emotional disturbance in infants.

Environmental factors influencing the emotions cannot be reviewed at all fully without covering the whole province of mental hygiene. Weill (139) has presented a review of recent discussions on this problem, together with data from 17 families purporting to show the dominant influence of environment. Blanchard and Paynter (12) have investigated the emotional reactions and family adjustments in a group of 80 children from economically "marginal" families. No evidence was found for an economic handicap affecting normal emotional adjustment. Blanchard (11) has elsewhere listed, in very general terms, four "outstanding factors which contribute to failure in socialization": lack of discipline in home, a broken home, lack of recreation, influence of bad companions. Gesell and

Lord (44), in a comparison of small groups of nursery school children from two widely separate economic strata, found at this pre-school age a reduced spontaneity and greater expressional inhibition in children from homes of inferior status.

VII. THE FUNCTIONAL SIGNIFICANCE OF THE EMOTIONS

Under this topic no attempt will be made to review more than a few recent illustrative references.

The expression of emotional disturbance in various forms of conduct disorder is of course a major topic of inquiry in the field of psychopathology. Evidence obtained by working backward from symptoms to assumed causes is commonly subject to error through lack of conclusive verification. This has often been pointed out in criticisms of psychoanalysis, but is no less true in connection with other types of clinical inquiry, unless particular care is taken in the matter of controls.

A representative example of inference from clinical observation, may be found in the recent report by Lowry (84), who discusses the effects of inferiority (in intellectual, social or physical traits); these effects may be manifested in withdrawal, compensation by developing excellence in some other trait, or over-compensatory expression in such a form as pathological stealing or lying.

Lying as a function of conflict involved in various emotional situations has been studied by A. M. Carmichael (28). The material used was a collection of 1,200 descriptions of the reactions of six-year-olds. Instead of lying, frequent alternative responses were crying, or acts of submission or combativeness. The fear motive as contributive to lying has also been emphasized by Baumgarten (6). Riddle (107) has reported on stealing as a form of aggressive behavior.

In a study of 7,664 clinic cases, Starr (120) has found among pre-adolescents only a very small percentage with a record of emotional disturbance. With adolescent delinquents, however, emotional maladjustments were shown to be of major importance.

The influence of emotion upon the nutritive processes in children has been made the subject of reports by Mohr (90), Aldrich (2) and others. J. L. Kantor (68), in a discussion of neurogenic and psychogenic alimentary disorders, gives a comprehensive bibliography of 77 titles, dealing with this subject, with titles up to 1928.

Emotional factors in the causation of enuresis have been frequently discussed in the clinical literature. A recent example of a statistical study is that of Ackerson and Highlander (1), who reported a low positive correlation between enuresis and such personality problems as irritability and seclusiveness, in a clinic sample of 3,000 cases.

The influence of emotion upon mental test performance has been studied by Jewett and Blanchard (54) and MacKaye (85). In the case of infants, Levy and Tulchin (80) have reported resistant behavior in over one-third of the subjects during mental test situations.

The relation of emotion to disorders of the language functions involves a literature so large as to require separate treatment. The most recent bibliography on speech pathology is that made by L. E. Travis (131) in 1929.

Both the behaviorists and the psychoanalysts have emphasized the importance of the emotional experiences of early childhood. Watson (136) has stated, "At the age of three the child's whole emotional life plan has been laid down, his emotional disposition set." On the other hand, Judd (67) and others have commented on what they consider the current over-emphasis of childhood experiences. In this, as in so many instances in the discussion of emotions, the literature is more richly supplied with speculative fancies than with actual data. Undoubtedly the chief present need is a scheme of research which will organize our methods and personnel towards a more systematic attack upon genetic problems.

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SPECIAL REVIEWS

DE LAGUNA, GRACE ANDRUS. *Speech: Its Function and Development*. New Haven: Yale University Press, 1927. Pp. xii+363.

The author divides her book into three parts. Part I deals with the social function of speech; Part II gives "a preliminary account of the general psychological scheme" which is the basis of Part III; the latter discusses the function of speech in the life of the individual. We may conveniently treat the book as attempting two tasks: first, that of presenting a thesis concerning the function and development of speech; and secondly, that of developing certain psychological principles. Let us consider the latter first.

In her preface, the author, in acknowledging her indebtedness to the philosophical writings of Professor Edgar A. Singer, Jr., writes, "The general behavioristic position adopted in the present inquiry is, I venture to believe, substantially the same as that which he was the first to formulate and which he has so brilliantly defended." The reader is therefore prepared to find the author's psychological basis dominated by two important features of Professor Singer's psychological theory; namely, the concepts of teleology and of "mind as behavior."

To the reviewer, the concept of "mind as behavior" suggests the figure of a number of shells, under certain of which, he is given to understand, "mind" is to be found; an understanding which he can however never verify; for as the clever operator raises each shell, there is nothing to be seen but behavior. This impression of the concept is strengthened by Professor de Laguna's development of it. She begins by pointing out the futility of that type of "empiricism" (so popular with the writers of current textbooks) which consists in avoiding all explicit statement of theoretical assumptions. She brings to view the metaphysical befuddlement which has led psychologists to call the subject of an experiment an *observer*. She declares for the method of behaviorism and says, moreover, that "it is only by definitely abandoning the assumption of a dualism that our present investigation is made possible." But it turns out that for her, "As a metaphysical theory, behaviorism replaces the dualism of mind and body with a monism, and endeavors to interpret

consciousness in terms of the organism and its behavior. With behaviorism as a metaphysical theory we are not here concerned, except to note its relation to psychological method. We shall not ask whether ultimately there is or is not any distinctive 'mental' existent, or whether the monism which behaviorism advocates is or is not 'materialism.' These questions are fundamental questions, and would merit the most careful consideration were our purpose less special. Our concern is with behaviorism as a theory of psychological method. As such a theory it stands for a complete and thoroughgoing description of mental phenomena in terms of objective conditions, including under these both the organism and the external surroundings in which it lives and acts" (page 127f). Concerning the above I would remark, first, that to apply the term *behaviorism* either to the metaphysical activity of "interpreting consciousness" or to the methodological activity of "describing mental phenomena" would strain that sadly overworked word beyond its limits; and secondly, that however special our purpose, to refuse an answer to the question whether we assume or do not assume ourselves to be dealing, ultimately or otherwise, with a "distinctive mental existent" is hardly to make explicit our principles. Nor do I believe that the scientific psychologist can safely continue to regard his fundamental postulates as belonging to "metaphysics."

The concept of teleology appears in the definition of a response as "not a definite bodily movement regarded as a change in spatial position, but rather a movement, or series or complex of movements, taken in its objective relation to some more or less distinct end which it normally serves." It is "adaptive with respect to specific factors in the environment." Locomotion and the throwing out of pseudopodia, *e.g.*, are not responses because they serve no single end, but occur as parts of many different responses or even occur *aimlessly*. "The fish often swims aimlessly about when he is not actively in pursuit of food or in flight from an enemy." It is not clear that the author recognizes this to be a fact, not of fish but of human psychology. Regarding the fish as a fish, we must recognize that under continually varying vital conditions continually varying responses are occurring, and that in all of these responses the same fundamental principles and mechanisms are involved. "Pursuit," "flight," and all other "ends" are linguistic classifications of responses with respect to their status in *human social interactions*. The author justly declares that "what as psychologists we

are primarily concerned with is not the physiological process as such, but the *identity of functional relationship* between varying processes," and further, "Psychological science, like every other science, must discover and formulate its own principles of individuation." But we ought to become clear that we do not "construe the acts of the organism in their relation to the physical world, and in so doing establish their psychological status"; that "ends" do not exist in nature apart from human interactions; and that the measuring device which we actually use in determining the "psychological status" of a response is the response which it elicits in other human beings. It is by means of this measuring device that we arrive at what Weiss has termed "biosocial equivalences."

Human behavior is described as characteristically made up, not of fixed type-responses, but of "complete acts," which are built up under varying conditions of individual life out of innumerable "functionally independent elements." The mechanism by which this organization proceeds is that of conditioning; the author, however, although describing the process (in a manner similar to that adopted by Smith and Guthrie), does not use the term; instead, she prefers to speak of an "affective transformation" of a stimulus. If we seek to determine what is meant by an "affective transformation," we find it said that "in so far as any stimulus tends to set up activities which normally lead to its own continuance, it is positive, or pleasant"; and further, "we are not to suppose that the pleasantness or unpleasantness is something additional to the behavior in question, a further consequence—or cause—external to the activity in question." But in the discussion of the development of speech in the child, we find repeated references to the "delight" and "enjoyment" of the child in its vocal activities; it is not easy to persuade oneself that no causal efficacy is implicit in these terms. So, too, in the discussion of "Space Perception in the Child" we read, "The sight and touch of the pile of blocks rouse in him vague longings.

. . . By accident, at first, he brings two blocks together in a way that pleases him," etc. Such description has an air of "sympathetic insight," but it does not really contribute to the scientific *analysis* of the processes involved. Similar remarks might be made concerning the alleged rôle of the "representative image." This curious changing of the basis of analysis at certain points seems to the reviewer a direct consequence of the author's separation of "metaphysical" and "methodological" postulates.

The author devotes most of Part II to an analysis of the factors whereby behavior, in phylogenetic and ontogenetic development, becomes more and more assimilated, in specificity and complexity, to the specific objective features of the environment. The lower animals have a few type-responses, adapted to a few categories of environmental situations; man has a large number of specific responses to specific features, including such "properties" as length and shape. The author shows the importance of the social factor and of the use of tools in developing responses to objects *in their relations to other objects*, rather than in their relations merely to oneself. But throughout this discussion the author seems to envisage three correlative lines of development: as *behavior* becomes more specific, so too does *perception*, so that there is an increasing development and "objectification" of the "*psychological environment*." The methodological advantage of repeating the analysis of the first of these in the names of the other two is not clear. It is only in a figurative, and hence in a scientifically bad sense that the psychologist can be said to study the development of the environment.

In Part I, the author attempts to trace the development of speech from the animal cry. It is assumed that the primary function of both cry and speech is that of coördinating the activities of the members of the group, and that the evolution of speech from the cry was guided by the necessity for a more flexible type of group organization under changing conditions of life, as in the assumed change from arboreal life to ground-dwelling. This notion of the basic function of speech is contrasted with the traditional view that language is fundamentally the means of expressing or communicating "ideas"; Wundt's doctrine of "expressive movements" is taken as an example of the sterility of this view which proceeds from the pre-conceptions of a metaphysical dualism. The work of Whitman and Craig on the behavior of pigeons is taken as a basis for an analysis of animal cries. The distinguishing feature of animal cries is said to be their direct connection with specific type-responses. "Instead of saying that they express feelings or emotions, we may say that they occur as elements of such responses." Similarly, the cry as a *stimulus* evokes chiefly type-responses in other individuals. Speech, on the other hand, does not occur as a mere element in a larger response, nor does it release one of a few large and relatively uniform modes of response. "The correlation between the speech-

response and its objective conditions is a correlation between independently variable elements of response and independently variable elements of the external situation, or of past or future events." The evolution of the basic *structure* of language from the animal cry has been conditioned by the increasing complexity of coördination in group life. The author believes that the evidence afforded by gesture-language, hybrid jargons, and the speech of the child (especially the "spontaneous" child languages) points to "a certain priority of a structure analogous to the isolating type." Explicit predication arose "from the combining of formerly independent terms." These formerly independent terms were *sentence-words*, in which predication was only implicit. The differentiation of words as units of analysis resulted from the extension of terms from one situation to others having objectively common properties. A typical way in which predication may have become explicit would consist in substituting for the bodily orientation or pointing which accompanied an implicitly predicative proclamation another utterance which designated the general situation. This evolution of the developed sentence structure was brought about by (1) the need for coördinated action beyond the limits of the common perceptually present situation, and (2) the need for coördinating such complex and varied behavior as depends on analysis of the situation.

The merit of this presentation lies in the author's clear exposition of the fact that vocal responses are in their biological origins of the same nature as all other components of pattern-responses; they are indicators, not of "feelings," or "emotions," or of "ideas" crying for expression, but of the nature of the total responses of which they are merely components. Being auditory stimuli, they readily become substitute stimuli for other individuals. In putting this matter clearly, the author has performed a real service. But her analysis of the subsequent development of true language, with its differentiation of structure, is of more doubtful value, and here again it is the intrusion of the concept of teleology which seems to interfere with a useful analysis. At every point the author seeks to show that the assumed developments fulfilled certain "needs"; that the requirements of social control "demanded" this or that development; whereas from the scientific point of view we are much more interested in isolating as many as possible of the *antecedent* conditions which made inevitable the developments in question. These antecedents are on the one hand the biological properties of human beings, particu-

larly the modes in which their behavior undergoes variation, and on the other the environmental, particularly the social, stimulating conditions to which they are subjected. However useful we may demonstrate language or any particular development in linguistic structure to have proved itself, we do not thus arrive at those laws of behavior which will alone constitute the scientific solution of the problem of the origin and development of language. It is not even clear how such an exposition will help us in the formulation of research problems.

Of chief interest in Part III is the analysis of the developments in individual behavior which result from the elaboration of *conversation*, which is regarded as "primarily the preparation for concerted or socially determined action." Two phases or stages in conversation are distinguished. "The first is the determination, through analytic description, of *what* the situation is in which action is to take place. The second is the formulation of a plan of action." The internalization of the first phase is regarded as the basis for belief and thought; the internalization of the second, as the basis for "conscious purpose." Free memory and imagination are held to result from the freedom and independence of (internal) speech, the course of which in these cases is usually directed by "desire." It is not clear whether the author means by "desire" a psychical force, interoceptive excitation, or verbal self-stimulation.

The "autonomous" nature of language is excellently presented. In current discussions of language by psychologists chief emphasis is usually placed on the *substitutive* nature of speech responses, and the fact is not always sufficiently recognized that language has become an activity in and for itself, consisting of responses far more complex, numerous, and specific than the repertoire of manipulating responses, and organized in structural patterns peculiar to the various languages.

In its emphasis on objective, genetic, and social factors, this book gives a good idea of the progress that the psychology of language has made since the publication of Wundt's *Völkerpsychologie*.

University of Washington

ERWIN A. ESPER

PURDOM, T. LUTHER. *The Value of Homogeneous Groupings*. University of Research Monographs, 1928, No. 1. Pp. 96.

The value of homogeneous grouping on the basis of intelligence was investigated in six high school groups. The first year pupils

were estimated to be approximately equal as to age, sex, intelligence score and the fact that they were taught by the same teacher. Each school provided a control group and experimental group divided into two to five sections, depending upon the size of the school.

The intelligence tests and the English and Algebra Achievement Test Scales were chosen on the very doubtful basis of volume sale. The teachers were instructed to teach "all she could of her subject with entire freedom of judgment as to method."

The author lists some eight advantages ordinarily accredited to homogeneous grouping: relating to variation and method of teaching different levels of intelligence, encouragement and progress of pupils, ease of instruction and enrichment of curriculum. Complete tables of distribution are given for each school with a summary of results of five schools studied. The gain in the standardized tests in terms of mean and median scores proves no advantage in favor of the experimental group; however, both English and Algebra semester grades were in their favor.

Teachers were asked to express their opinions on the basis of eleven questions. The opinions were greatly divided and did not correlate with the results attained in their sections. This was especially true in the case of the progress of dull pupils: where teachers felt they had gained most, the control sections maintained better standing.

He concludes that pupils in homogeneous sections neither gain more when results are measured by standardized tests, cover more course material, nor put forth greater effort. These results hold true for pupils of varying degrees of intelligence measured by standardized tests.

The study ignores almost entirely the analysis of the student's reaction or his motivated attention. The value of such grouping as an incentive to study should be investigated upon a basis of more definite measurements. Actual control sections must also be established by identical teaching material and procedure.

University of Pennsylvania

R. A. BROTEMARKLE

COCKS, A. W. *The Pedagogical Value of the True-False Examination*. University of Research Monographs, 1928, No. 7. Pp. 131.

The investigation reported attempts to measure the relative value of the true-false examination to the "teacher at work in the class-

room." The purpose of the study is three-fold—as to measurement, motivation, and instruction.

The immediate value of the true-false examination is said to be related to its form, procedure and scoring. Three first-year classes at the high school level were divided into two equivalent groups on the basis of mental age determined by the British Columbia Intelligence Test. The testing material consisted of an examination composed of two parts. Test 1 made up of fifty multiple choice or best-answer questions; and Test 2 made up of fifty true-false questions. Questions were based on the High School Physics course prescribed by British Columbia and the questions were divided proportionately in both tests. The pedagogical value of the true-false test is found to be more than 50 per cent greater than that of the multiple choice. A complete reexamination substantiated the original finding.

The author then attempts to estimate the relative value of true-false statements. He adds his opinion to that of others and concludes that the false questions are of some value even when not corrected by the student.

He finds that while the multiple choice test is of greatest benefit to the bright student, the true-false test is of equal influence among the dull and bright.

Similar results are secured for tests in spelling, algebra and chemistry. He concludes from his own study and that of others that the best method of scoring true-false examinations is to instruct the pupil "not to guess" and inform them that the score "will be right minus wrong"; except when relative standing alone is wanted, in which case the right score alone is sufficient.

The pedagogical value most emphasized, that of review, is not inherent in the form or the material or the test itself, but is secured through the process of correction at the hands of the pupil.

R. A. BROTEMARKLE

University of Pennsylvania

VAUGHAN, W. T. *The Lure of Superiority*. A Study in the Psychology of Motives. N. Y.: Holt, 1928. Pp. 307.

Accepting Adler's theory of Organ Inferiority and Compensation and adjusting the same to Nietzsche's "will to power" the author has developed "compensation" as the "resolution of conflicting impulses." Considerable importance is attached to the time sequence.

Two general basic types of adjustment are the physiological with its process of substitution of another function, and the psychological with its substitution in kind. Specific types, based upon the "organ" theory and types of defect, are the physical, mental, social and moral. These latter may be basically of either general type, with the resulting direct or indirect functional activity.

Illustrative materials are drawn from the social groups in which conflicts have been intensive and persistent. The lives of Schopenhauer and Lincoln are used as individual illustrative studies.

The author has collected the problems of "inferiority" and "compensation" without in any way solving either; and in fact without the clarity sometimes used by the original writers. One is startled by the credence placed in statements and interpretations found in editorial and reportorial comments of certain of our modern newspapers and weekly digests.

R. A. BROTEMARKLE

University of Pennsylvania

LEARY, D. B. *Modern Psychology, Normal and Abnormal. A Behaviorism of Personality.* Phila.: Lippincott, 1928. Pp. 441.

The author states that his purpose is to "first, present a sober and scientific basis for the study of the behavior of personality"; "second, to deal with, evaluate and classify personality on the basis of types and degrees of dynamic adjustment to the physical and social environment; finally, bridge the gap between physiology, as an atomism of human behavior, to such a synthetic point of view as may properly be called a behaviorism of personality."

One is astonished at the variety of new terms, phrases, and theories necessary to the unity of the "sober and scientific" viewpoints. This necessity arises from a constant emphasis on "synthesis" as contrasted with the "all too-prevailing barren atomism of current behaviorisms."

A new theory of animal and human learning is advanced on the basis of the "general theory of the conditioned response." A new scheme of personality classification is based on a "dynamic-evolutionary theory of personality," and established subdivided groups of adjustment types centering about "non-association," "compensation," and "surrender" or "the inhibited exercise of the socially non-adjustive patterns."

Part IV, under the heading of "Types of Personality Adjust-

ment" presents a new viewpoint of religion and philosophy in terms of the synthesis or integration of human behavior. "Sharp and definite lines of demarkation" are evaded to such a degree as to leave little definite in the new interpretation. Mysticism becomes behaviorism.

R. A. BROTEMARKLE

University of Pennsylvania

ORATA, PEDRO TAMESIS, *The Theory of Identical Elements*, etc. Columbus: The Ohio State University Press, 1928. Pp. xi+204.

The author announces that his study is a critique of the theory of identical elements as employed by Thorndike to explain transfer of training and that it also sets forth a reinterpretation of transfer which is felt to be in better accord with the facts. The monograph is mainly philosophical rather than scientific in spirit. Orata has a reason for hoping that the theory of identical elements is erroneous and he does not hesitate to make that hope plain. He feels that, if the theory were sound, educational practice would be called upon to move back to the apprenticeship system which is, in his eyes, incompatible with the democratic ideal. It is a little difficult to understand just what democracy has to do with the validity of a scientific theory, but, of course, if one's attitude is not strictly scientific, one may, in the interests of certain social values, hope for the downfall of an embarrassing theory.

Orata reviews a considerable number of experiments and finds that transfer is dependent not so much upon the number of elements in common as it is upon the possibility of the subject being aware of some general principle applicable to the related activities. He believes this contention to be supported by the fact that Judd, Ruger, Ruediger, Coxe, Woodrow, Meredith, and others, who provided for such consciousness of general principles, tended to get more positive evidence of transfer than did those who worked with activities at an automatic level. He sums up his review of experimental evidence by saying: "We have demonstrated by the results of experimental investigations that transfer takes place to the extent in which conditions favorable to transfer are present" (page 157). Apparently he feels that this statement contains the death sentence for the doctrine of common elements. It is difficult for the reviewer to see that it is of the least consequence for the quarrel at hand. Even Thorndike has probably suspected that transfer takes place when conditions are favorable for the occurrence of that phenomenon.

The author's reinterpretation is based largely upon an arbitrary act of definition. He says (pages 175-176): "We have assumed that there are two types of behavior or reaction corresponding to two kinds of environment. One is automatic, reflex, or mechanical habit, in response to an unchanging environment; and the other is intelligent behavior in a meaningful and highly modifiable situation. In our problem of transfer we are concerned only with the second type of behavior (intelligent) and the second kind of environment (meaningful)." Is it any wonder, when the author admits that he will call transfer only such transfer as occurs where there is conscious understanding of relationships, that he will find transfer only under such conditions? The dice are loaded; the victory is sure.

There are few topics in psychology that are more deserving of critical examination than the transfer of training. Its importance is at once fundamental and practical. The doctrine of common elements is only an approach to the problem. Two activities can have anything in common from neurones to mathematical principles. What is needed is investigation of the degree of effectiveness of various types of common elements. Vague charges that this approach is based upon psychic atomism and mechanism or that it is against the interests of democratic education get us nowhere.

EDWARD S. ROBINSON

Yale University

NOTES AND NEWS

THE election committee of the American Psychological Association has announced the election of the following officers: President for 1930, Herbert S. Langfeld; Directors, 1930-1932, John F. Dashiell and Arnold Gesell; nominees for appointment to the Division of Anthropology and Psychology of the National Research Council, 1930-1932, A. T. Poffenberger and Calvin P. Stone; representative on the Social Science Research Council, 1930-1932, John E. Anderson. Carl C. Brigham of Princeton University continues as Secretary and Edward S. Robinson of Yale University continues as Treasurer of the American Psychological Association.

THE Seventh International Congress of Philosophy will meet September 1-5, 1930, at Oxford, England. Communications should be addressed to the Secretary of the Congress, Mr. A. H. Hannay, 74, Grosvenor Street, London, W. 1, England.

THE First International Congress on Mental Hygiene will be held at Washington, D. C., May 5-10, 1930. Communications should be addressed to the Administrative Secretary, Mr. John R. Shillady, 370 Seventh Avenue, New York, N. Y.

EDITORIAL NOTICE

BEGINNING with the 27th Volume in January, 1930, there will be a change in the dates of publication of the *PSYCHOLOGICAL BULLETIN*. In the future, the *BULLETIN* will be published only ten times each year (the August and September numbers will be omitted) with an average of 72 pages per issue. Thus the present size of the volume of 720 pages per year will be maintained by increasing the size of the separate issues.

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